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ORGANISATION FOR SCIENTIFIC AND INDUSTRIAL RESEARCH

SIR EDWARD APPLETON, in his A. D. Little Memorial Lecture in 1946, distinguished three types of research : free fundamental research, prosecuted in universities and similar institutions, without any regard to immediate or future application ; objective fundamental research, designed to give insight and understanding rather than any special immediate practical result ; and, third, applied research. He emphasized that there is no sharp differentiation between these types of research, but he finds them a more convenient classification than the older division into 'pure' and 'applied' research. The second type is carried out, as well as applied research, in Government establishments ; some will also be carried out in industrial research laboratories, although in the main the latter will be carrying out applied research.

Now there are two points here of special importance in regard to the research associations which have led to doubts as to whether their expansion is wise without further inquiry and some experiment with institutions of the type of the Mellon Institute or the Institute of Paper Chemistry at Appleton, Wisconsin. As the Advisory Committee of the Privy Council noted in its first report in 1916, and as Dr. D. W. Hill points out in his recent book, a research association that fails to encourage and to inspire research activity on the part of its members fails in a vital part of its function. The research activities of the research associations may even be of secondary importance to the stimulus they give to independent research, and in assessing the value of the whole movement it is of prime importance to recognize the extent to which research has been stimulated in industry itself through their work. It is far more important to ensure that the contact and general relations between the research associations and industry, geographically and in other ways, are such as to ensure that there is the maximum interchange of information, ideas and inspiration, and that co-operation is real and not nominal, masked by payment of a subscription.

It is true that recent fiscal changes introduced by the Government may stimulate research by private firms in future, although for the moment the concession as regards capital expenditure may be largely ineffective ; but it is the more important to direct attention to these questions first rather than to the selection of problems for immediate conjoint research, because of the trend in so many reports from the 'working parties' of the Board of Trade to advocate a compulsory levy for research. There is real danger that even a voluntary subscription to a research association may act as a sedative and induce a reluctant firm to think that it has done all that is necessary, and take no further steps to ensure that it is staffed and equipped to utilize the results of research as they are made available.

More effective liaison work may do something to decrease the danger, and hence the growing import-

ance of the work of the information departments maintained by the individual research associations and centrally by the Department of Scientific and Industrial Research. Compulsory contributions are, however, likely to increase the danger, and for this reason alone experiment with the alternative type of organisation, the research foundation, might well be worth while.

That idea has received strong support in recent conferences; the Fulmer Research Institute, opened last July, is one of the first steps taken to fill a recognized gap in the organisation of research in Great Britain, and in welcoming this initiative, Sir Stafford Cripps, then President of the Board of Trade, expressed the hope that the Institute would be the forerunner of others on similar lines which might largely fill the gap altogether. At the Scottish Regional Conference of the Federation of British Industries in Glasgow last April, Prof. R. Hay suggested the establishment of a foundation for Scotland on Mellon Institute lines, and in view of Sir Edward Appleton's remarks at the Manchester Conference on "Research and the Smaller Firm", it would appear that concrete Government support may be forthcoming for particular proposals of that kind. While the organisation of research in Great Britain is still fluid, every opportunity should be utilized of making such experiments before too heavy a proportion of the nation's resources for research are committed to particular forms that may prove to be not the most desirable.

The importance of research organisation is well brought out in a recent survey of German research institutions by Col. L. E. Simon*. According to Col. Simon, German war research was defective in three respects. It failed to plan basic research with appropriate anticipation of future requirements for development—much time was wasted on stop-gap measures; it did not provide an adequate development echelon which was independent of self-interest; and intelligent communication between research units and the German army was lacking. For such reasons, in spite of the quality of much of their basic and technical research and the excellence of design, German research on war weapons was markedly inferior to that of Britain or the United States. Defective leadership and absence of effective means for technical co-ordination, coupled with poor development, in Col. Simon's view, probably reduced the effectiveness of German research and development by fifty to ninety per cent.

That verdict alone invites a closer scrutiny of the structure developed in Great Britain for war purposes, and particularly of what is usually described as 'operational research', with the view of seeing whether any features of the latter should be incorporated in our peace-time system. Before doing so, Col. Simon's observations on the institute as the fundamental unit in the organisation of research are worth noting. In view of the proposals made in Britain for development on such lines, it is of interest that Col. Simon commends the idea for trial in American industry, and he outlines with some care the factors to which its success in Germany is attributed. The institute should not be larger than one man can administer effectively through group leaders only one echelon above the research workers themselves, and in practice this means a staff not exceeding three hundred in all. The leader in an institute of this size should not tend to become an administrator instead of a scientific leader. Obviously, there are other questions of organisation, such as the relation of the institute to other research units and to industry, the universities or departments of State; but Col. Simon seems to think that such a unit has inherent possibilities of avoiding the danger which Sir Henry Dale emphasized and to which Mr. John Mills, lately director of publications at the Bell Telephone Laboratories, also directed attention in his recent book, "The Engineer in Society", that of diverting our ablest scientific minds from the creative work of research into administrative work which could be done by others less fertile and imaginative, even if they too require some appreciation of the scientific attitude.

For the rest, what Col. Simon notes about the conditions most conducive to creative research confirms what was emphasized in the Barlow Report on Scientific Staff and in other reports which have appeared in Britain and in the United States during recent years. To most of these considerations Government, the universities and industry now appear to be fully alive, and it is to be hoped that restrictions imposed by the economic crisis will not be allowed to endanger the situation. There is, in fact, at least some reason for concern at the apparent lack of appreciation on the part of the Government as to the importance of incentives for research workers, even if the threat is indirect rather than direct. Many of the general restrictions already imposed from quite other considerations have a detrimental effect on efficiency which, by engendering a feeling of frustration, tends to undermine incentives and destroy the atmosphere in which creative work is most fruitfully pursued.

One other factor noted by Col. Simon may also be mentioned—that of scientific intercourse and the exchange of ideas—and this stresses the value of the new intelligence group of the Department of Scientific and Industrial Research, some of the research stations of which may well be regarded as research units of the type mentioned above. Into the same category would fall a number of the research associations, such as the British Cotton Industry Research Association, which maintain a central institution; but we are here concerned with a factor which does not appear in Col. Simon's analysis. The units he describes are occupied essentially on fundamental research or the search for new knowledge of a broad or general character, and only to a lesser extent with technical or engineering research as he terms it.

The comments in the third report of the Select Committee on Estimates as well as the establishment of the Advisory Council on Scientific Policy warrant a hope that an inquiry will be held into the possible types of organisation for co-operative industrial research.

* German Research in World War II: an Analysis of the Conduct of Research. By Colonel Leslie E. Simon. Pp. x+218. (New York: John Wiley and Sons, Inc.; London: Chapman and Hall, Ltd., 1947.) 24s. net.

There is a further matter relating to the expansion of the research association movement which requires consideration. A number of the research associations have already recognized that the utilization of the results of research by an industry depends on the presence within the industry of men able to appreciate the significance of these results and to apply them in practice. That has been repeatedly stressed in reports from the 'working parties'. The most effective way of strengthening an industry is by the recruitment of young men of wide educational background and of a scientific attitude of mind. The 'working party' for the lace industry in its recent report gave close attention to this question of recruitment and training, and pointed out that the presence of such men in the industry would assist it to make the best use of the results of research and also help to render fruitful the investigations carried out by individual firms.

No attempt was made in this report to indicate how such recruits should be introduced to the lace industry, and one aspect of the research association movement or the research institute which needs to be studied more closely is its success in supplying industry with men with a scientific training and research outlook, and in securing the interchange of staff. This, with such an industry as the lace industry, is just as important as securing close contact of the association or institute with the industry it primarily serves; one important factor in the success of the Institute of Paper Chemistry at Appleton, Wisconsin, is probably its location in a compact area of paper mills and paper-mill machinery manufacturers, and its association with Lawrence College, so that teaching and research are associated as in a university. Once this problem of the introduction to the industry, particularly at the managerial and administrative level, of men of a scientific outlook, qualified and competent to appreciate the contribution of science, is overcome, there will be little difficulty in formulating appropriate programmes or targets for research. But unless it is done, and the industry becomes thus qualified to interpret and apply, and even in some degree to direct research, there appears to be real danger that the measures contemplated by the lace industry 'working party' in regard to research and development may have only a passing effect, and fail to produce the sustained effort and the change of outlook that are imperative.

We have to remember that the traffic between science and industry must be in both directions, and the research association or the research institute, like the research department of an individual firm or of a Government department, must look not only to the manufacturing department or works, but also to the universities and other centres where the frontiers of knowledge are being continually advanced. The importance of this has indeed been strongly impressed on us by our war-time experience. Sir Edward Appleton referred in his lecture to the brilliant success of university men of science in solving war problems entirely remote from their peace-time interests, and to the way in which university conditions generally favoured the maintenance of the mental adventurousness and lively imagination so necessary for scientific progress. Possibly the regional research council might make an important contribution here.

The Interdepartmental Scientific Panel is clearly seeking to ensure that conditions in the future Scientific Civil Service of Britain encourage these same qualities, but in the meantime the brilliant success which has attended the close collaboration between scientific worker and Service man already noted has led to the suggestion that the same type of collaboration should be organised for the attack on peace-time objectives. The value of close collaboration between producer and user has long been recognized in some sections of industry, as is evident in the technical service to customers maintained by numerous progressive firms. In this dual flow of ideas there is a continuous improvement in the products of industry; and the value of contact between user and investigator and of all concerned in a research problem could scarcely be better put than by Dr. Price Russell in his lecture "The Organisation of Industrial Research" before the Industrial Research Committee of the Federation of British Industries.

The similarity between this and the basic idea behind 'operational research' is obvious, and Sir Robert Watson-Watt has made the pertinent remark that operational research is beginning to look very like real scientific management. Leaving for the moment the possible contribution 'operational research' may make to the still unsolved problem of the relationship between the administrator and the expert, and confining ourselves to the scientific and technical level, there should be noted the value, established in our war-time medical research, of a section composed of independent, full-time and fully mobile observers; and the conclusion that minority opinions should not be ignored even when counter to expected results. To both of these points Prof. W. C. Wilson has directed attention; but much careful thought will be required as to the means by which the first feature, which has something in common with the nuclear group described by Sir Percy Dunsheath, can be incorporated in our existing organisation.

In the main, however, those who, like Prof. J. D. Bernal, are anxious to see the technique of 'operational research' adopted in our peace-time structure, are thinking of doing so at a higher level, and with the view of seeing that Great Britain's limited resources in engineering and science are used in the most effective way possible, having regard not merely to the existing situation, but also to the way in which resources could be very considerably and rapidly increased. To some extent that is a matter of providing what Dr. R. P. Russell termed the political climate of research; but apart from what could be done at that level, it rests largely with scientific workers themselves to ensure that so far as organisation is concerned the channels of communication are adequate and effective, and that there is a free flow of workers and ideas between the universities, industry and the departments of State. New departures, such as the diversion suggested by Dr. Wansbrough-

Jones of young scientific men really interested in the social applications of science for a short time to 'operational research', may well assist. With adequate safeguards it might be an advantage to many to work for a year or so, say in their late twenties, for a Government department; to leave industrial laboratories to work on the scientific man-power side of their industries, and defence laboratories to work with the Services.

These are all tactical measures designed to increase mobility and, by facilitating the interchange of staff and ideas, to foster that cross-fertilization of minds which is so fruitful of real scientific advance. If such measures are to succeed, however, they must secure the cordial support and the careful and unceasing criticism of men of science themselves, individually and through their professional associations. They must be quick to note and to make representations against any developments which, by over-organising science, or, for example, hindering the vital freedom of the investigator to single out the right path to his objective in the complex situation existing on the frontier of knowledge, and to resist encroachment from the administrative side as to the methods to be adopted and/or the particular paths to be followed in the search for solutions of scientific problems. In his presidential address to the Royal Society Sir Robert Robinson indicated that this particular danger is now very slight. Even the selection of problems for investigation cannot be made wisely without the participation of the scientific investigator himself; and the most disturbing features in both Sir Edward Appleton's lecture and the report of the Select Committee on Estimates are the indications that in some departments of State all is not well in this respect, and that there is still failure to identify departmental problems suitable for scientific treatment, to see that these problems are passed to the appropriate research bodies for handling, and to interpret the incoming scientific material for the special departmental purposes.

It may be the function of the Advisory Committee on Scientific Policy to remedy defects in departmental organisation in due time; but no matter how efficient the organisation for research which is ultimately developed for the nation's purposes, in the sphere of Government itself, in the universities or in industry, no matter how well balanced the distribution of man-power between the different fields, or adequate the financial endowments and other material resources, the benefits the nation derives will depend ultimately both on the quality of the men of science engaged in the research and of those who, whether or not they are themselves men of science, are responsible for the administration. It is appropriate that at this present time the whole organisation and expenditure of the nation on research and development should be subject to the closest scrutiny, that the impact of restrictions on capital development should be examined, and that the possibilities of new types of organisation for research and new techniques for co-operation should be explored. There can be no more finality in the organisation than in the results of research.

MATHEMATICS OR PHYSICS?

Conduction of Heat in Solids

By Prof. H. S. Carslaw and J. C. Jaeger. Pp. viii+386. (Oxford: Clarendon Press; London: Oxford University Press, 1947.) 30s. net.

CARSLAW and Jaeger have produced an interesting book, different parts of which will meet the needs of mathematical physicists, experimental physicists, university teachers and design engineers. It is didactic rather than exploratory. In plan it owes much to Carslaw's "Introduction to the Mathematical Theory of the Conduction of Heat in Solids" which appeared in 1921; but the volume now under review differs so much from the earlier one that it must be considered as a new work rather than a revised version of the older one. It embodies many advances that have been made during the past twenty-five years, as can be seen from the numerous references to mathematical, physical and engineering publications, many of which have been published since 1921. The references also indicate that while attempting to give the book a pronounced mathematical flavour, the authors have also tried to make it useful to engineers and physicists. In addition, some essentially 'practical' topics are treated briefly in the text; of these some of the more interesting ones are: cooling fins, the conduction of heat in a thin wire heated by an electric current, automatic temperature control, heat regenerators and heat exchangers, the heating of a chain of radiating slabs, etc. No attempt has been made to treat these topics in great detail, but references are given to the relevant further literature.

The preparation of the book must have presented the authors with almost all the types of problem confronting those who write on branches of physics that lend themselves to detailed mathematical treatment. Should the subject be treated as a physical one, and the mathematical techniques of solution relegated to appendixes or to sections in smaller type? Should there be a discussion of fundamental principles, or should it be assumed that readers will be familiar with them? Should the book be written for research workers, or for teachers and students? There is no doubt that the authors were aware of these questions and that they decided to assume that the basic physical ideas were well known. Problems were grouped and discussed according to the mathematical techniques employed in solving them. For example, after an introductory chapter (1) dealing with the derivation of the equation of the conduction of heat, the appropriate general theory, boundary conditions, uniqueness of solutions, etc., there follows a sequence of chapters (2–9) in which are grouped problems that involve the flow of heat in an infinite and semi-infinite solid (2), solids bounded by two parallel planes (3), in a rod (4), in a rectangle (5), etc.

At this stage, the mathematical technique of the Laplace transform is introduced (11). This chapter contains an outline of the history of the development of the transform, of its fundamental mathematical properties and of its application to problems involving the linear flow of heat. The remaining chapters (12–15) deal primarily with the application of the transform to problems more complicated than those discussed earlier in the book. The authors point out that the Laplace transform could have been introduced earlier in the text, and that such an

early introduction would have simplified much of the previous chapters. They felt, however, that the juxtaposition of the classical technique and the more modern one should be very helpful, and the reviewer agrees with them wholeheartedly. Six valuable appendixes conclude the text.

The book would have been more satisfying had more space been devoted to brief critical accounts of physical principles and definitions. For example, the orders of magnitude of the variation of conductivity and specific heat with temperature might have been discussed and, where known, the results of taking these variations into account might have been compared with the result of assuming these quantities to be constants. Or again, the note on the effect of turbulence on the coefficient of outer conductivity might have been amplified.

The authors have written on a difficult subject. They have formulated their plan with care and have kept to it. Others, including the reviewer, might have preferred a more physical approach without the sacrifice of any of the mathematical ingredients of the publication, but this is purely a matter of opinion. Carslaw and Jaeger are to be congratulated upon this addition to the literature of the conduction of heat in solids, and note should be taken of the compliment paid by Prof. Carslaw to his junior colleague, Dr. Jaeger, when he says in the preface "He is most certainly its principal author".

This review would not be complete without a special note of praise for the printing and binding. Those whose work involves a close scrutiny of scientific texts are indebted to the Oxford University Press for the continuation of its high standards even in times of great difficulty.

L. ROSENHEAD

THE NEUROTIC PERSONALITY

Dimensions of Personality

By Dr. H. J. Eysenck. A Record of Research carried out in collaboration with Dr. H. T. Himmelweit, Dr. W. Linford Rees, and with the help of Dr. M. Desai, W. D. Furneaux, H. Halstead, Dr. O. Marum, M. McKinlay, A. Petrie and P. M. Yap. Pp. xi + 308. (London: Kegan Paul and Co., Ltd., 1947.) 25*s*. net.

THIS book is based upon research carried out by a team of psychologists working at the Mill Hill War Emergency Hospital. The object of their investigations was to discover the relationship of various traits of personality to 'neuroticism'. The experiments were performed on a group of patients, numbering seven hundred, suffering from reactive types of mental illness. The statistical method was used, and careful correlations are given for the various traits.

The authors state that two main facts emerge from their study. First, there is a general factor, 'neuroticism', shown by signs of a badly organised personality, poor muscular tone, pre-service evidence of abnormality, abnormal parents and unsatisfactory home-life. Secondly, a bipolar factor with affective type of symptoms (anxiety, depression, obsessional tendencies, etc.) at one end and hysterical type (conversion symptoms, sex anomalies, low intelligence, bad work history, hypochrondriasis, etc.) at the other.

A number of studies were made upon physique and constitution, such as body build, autonomic activity, dark vision, choline esterase secretion and exercise response. The authors claim significant differences between the two groups. Other studies were made upon neurotics and non-neurotics with regard to intelligence, accuracy of work, level of aspiration and so on. These tests are claimed to differentiate dysthymics from hysterics and normals from neurotics.

Tests were also made upon various types of suggestibility and the relation of them to hysteria. The authors believe that there is no evidence that hysterics are more suggestible than other neurotics. They found, however, that suggestible patients were made more so by narcotics, whereas non-suggestible ones remained unaffected. In the experiments made upon appreciation and expression it was found that neurotics were more reactive to colour, but there was no difference between the hysteric and the dysthymic.

These tests may arouse some readers to complain that the mind is so fractionated in such a study that nothing much is proved. Prof. Aubrey Lewis, in the brief foreword, answers this objection that the methods are 'atomistic', and points out that it is an attempt at "the analysis, by reliable statistical techniques, of experimental and clinical data, so that measurement may be possible and a sight obtained of the promised land where mental organisation will be as well understood as the physical organisation of human beings is".

This research is interesting, and the frequent quotations show a wide knowledge of the literature. A few show an unnecessary desire to quote from other Maudsley colleagues, as, for example, that on page 51: "A person's constitution may render him liable to breakdown at the slightest provocation; on the other hand, a person of first-class constitution may finally break down when the stress is too great." This is such a psychiatric commonplace that surely it has been stated many times before Slater in 1944. These, however, are minor blemishes and the book as a whole is valuable.

The format is neat, the paper good and the printing clear. It is written as easily as such a subject allows. There is a useful summary of the findings at the end of each chapter and many diagrams clarify the text. There is a long bibliography of 24 pages and an excellent index.

CLIFFORD ALLEN

"CUSHNY'S PHARMACOLOGY"

Pharmacology and Therapeutics

Originally written by Prof. Arthur R. Cushny. Thirteenth edition, thoroughly revised by Prof. Arthur Grollman and Dr. Donald Slaughter. Pp. 868. (London: J. and A. Churchill, Ltd., 1947.) 45*s*.

WHEN Cushny's text-book first appeared in 1899, it was recognized as being "the first severely critical, rigorously scientific general text-book to be written in English by an experimental pharmacologist". It helped to put the science of pharmacology on secure foundations by eliminating traditional lore and concentrating attention on proved facts. It became the standard text-book in English on both sides of the Atlantic, and Cushny himself was responsible for eight editions. After his death in 1926, four more editions were prepared jointly by Prof. J. A. Gunn of the University of Oxford and Prof. C. W. Edmunds of the University of Michigan. The thirteenth edition, which has recently appeared, is a purely American production. Most of the work of revision has been done by Prof. Arthur Grollman,

who is professor of both medicine and pharmacology at Southwestern Medical College, Dallas, Texas, and is well known for his work on the circulation and on the adrenals. The section dealing with depressants of the central nervous system has been revised by Dr. Donald Slaughter.

The revision has been thorough and must have been a vast labour. Pharmacology is advancing rapidly and nearly all the new advances have been included. It is unfortunate that new revisions of both the British and United States Pharmacopœias are appearing too late to influence this edition, but the pharmacological information is well up to date. Among the newer drugs which appear are streptomycin, folic acid, "Paludrine", tridione, *B.A.L.*, *D.D.T.* and *D.F.P.* (these initials mean as much to the pharmacologist as the full chemical names). There are many references to work published in 1946, but the material must have gone to the printers fairly early in the year or the formula of synthetic folic acid would undoubtedly have been included. In spite of many changes, the general arrangement has not been altered and the critical spirit has been maintained; there is rather more emphasis on practical applications and rather less emphasis on academic experiments with isolated organs.

Text-books almost always show a certain amount of national bias, and it is interesting, and perhaps a little sad, that this one now shows some slight signs of American bias. The section on anthracene purgatives was apparently written in ignorance of Straub's work on senna. Benadryl and pyribenzamine are mentioned, but not antergan and neoantergan. Benzestrol is included, but not diencestrol. Lanatoside *C* takes the place of digoxin. Most of these differences are superficial and merely serve to illustrate the fact that new discoveries may be applied in slightly different ways in different countries. There can be no doubt that this new edition of Cushny's book maintains a high standard and will be an invaluable source of information both for students and their teachers. Few students will read it all but many will keep it for reference, and it can be warmly recommended for this purpose. J. H. GADDUM

POPULATION OF THE SOVIET UNION

The Population of the Soviet Union

History and Prospects. By Frank Lorimer. (League of Nations Publications. II. Economic and Financial. 1946. II. A.3.) Pp. xiv + 289. (Geneva: League of Nations; London: George Allen and Unwin, Ltd., 1946.) 15*s*. net.

KNOWLEDGE concerning the U.S.S.R., apart from political opinions of various colours, has been hard to get. We have had invaluable essays, notably by Sir John Maynard and Sir Bernard Pares, but there has long been a serious need of more information than such reference books as the "Statesman's Year-Book" could find space for. This gives a special value to the publication under review. Dr. Lorimer studies and provides not only the available facts of population and employment, but also, by detailed comparison of data for 1926 and 1939, reviews the dynamics of population changes and movements during this period of vital change.

A Russian population roughly estimated in 1724 as 17,900,000 reached a total of 58,629,000 in 1859, by which year Russians in the Asiatic part had reached 3,424,000. In 1897 the figure was 94,331,000 Russians (6,947,000 in the Asiatic part), to which must be added more than 31,000,000 non-Russians, making in all 125,640,000. For 1914 the total is estimated at 142,389,000, but the terrible years 1917–23 reduced it to 136,102,000, since when it was on the up-grade until the recent War. It passed its 1917 figure in 1926–27, with 11,802,382 east of the Urals. In the 1939 area of the U.S.S.R. there were at the end of that year 173,788,000. We thus have a continuation of a large percentage increase. Projections of population are given which show that war losses in numbers should be made up before 1950, and the 200,000,000 mark passed before 1960. As the population of North-western and Central Europe is expected to decline after 1950, it is estimated that the U.S.S.R. will equal North-western and Central Europe in population by 1970, and is more likely to go on increasing thereafter. The age composition of the population is strikingly different from that obtaining in North-western and Central Europe (figures are given in percentages).

	Young people	Males 20–44	Males 45–64	Females 20–44	Females 45–64	Old people
U.S.S.R., 1939–40	45	18	6	20	7	4
N.W. and Central Europe	32	19	10	19	12	9
Estimated, U.S.S.R., 1970	35	20	9	20	10	6
Estimated, N.W. and Central Europe, 1970	21	18	14	18	14	15

Though it is anticipated that the expectation of life will increase, this extract from the tables shows that the proportion of young people is likely to remain far higher in the U.S.S.R., and the proportion of old people will be much less than elsewhere in Europe.

City growth is as marked in the U.S.S.R. as elsewhere. Moscow doubled and Leningrad nearly doubled during 1926–39, reaching more than 4 and 3 millions respectively. In 1926 Kiev had 513,637; by 1939 nine towns besides Moscow and Leningrad had topped the half-million. In 1926 about thirty other towns had 100,000 or more; in 1939 the number was 71. Figures from German sources for the period of the Hitlerian invasion naturally show enormous reductions, especially for Ukrainian towns.

The amount of sown land per head of the population not only kept pace with population increase in nearly all regions except Belorussia, Karelia, Murmansk and Azerbaydzhan, but actually increased, the average for the U.S.S.R. rising from 0·936 to 1·195 hectares, the increase in the Lower Volga and Don regions and Kazakhstan being outstanding.

The net reproductive-rate, as usual, is far higher in rural than in urban areas, and as the average for the whole of the U.S.S.R. in 1938 was estimated at 1·54, the rural birth-rate is very high indeed. In rural areas women between twenty and thirty-nine are responsible for nine-tenths of the births. In urban areas the number of births to women of less than twenty is slightly larger; those to women of forty and more are much less. The birth-rate in 1938 was as high as 38·3 per thousand and the death-rate was 17·8.

There are 22 maps, 103 tables and 31 figures, and they deal with occupations and industrialization as well as with vital statistics of many kinds. Readers will find the statements of fact invaluable, and perhaps scarcely need to be warned that projections suggesting future conditions are subject to a good many reserves. H. J. FLEURE

THE GEOLOGICAL SURVEY OF INDIA, 1846 TO 1947

By SIR CYRIL S. FOX
Formerly Director, Geological Survey of India

NOW that the Empire of India has ceased to exist as a British possession and the control of the services in Hindustan and Pakistan has passed from Whitehall, the records of many British-organised services will be reviewed and then perhaps forgotten. I had attempted to tell the story of the Geological Survey of India ten years ago in my presidential address to the Mining and Geological Institute of India (*Trans. Min. and Geol. Inst. India*, **31**; 1936). It was evident to me then that the beginnings of the Geological Survey of India were not correctly known, and certainly not as represented in the record by Sir Clements R. Markham ("Memoirs of Indian Surveys", p. 216; 1878), nor as given by Sir Thomas Holland in his "Indian Geological Terminology" (*Mem. Geol. Surv. India*, **51**, Part 1; 1926). I began a search for information in India, first among the files of the Geological Survey of India, then in the Record Office in the Secretariat of the Government of Bengal, and finally, on my return to Britain last summer, in the record room at the India Office, Whitehall. The correspondence relating to the engagement of David Hiram Williams and, eventually, after his death, to that of Thomas Oldham and his re-engagement after five years service, show that Oldham was recruited as "successor to Williams", and that, even at the time of his re-engagement by the Honourable Court of Directors of the East India Company, Oldham was addressed as "Geological Surveyor". These facts are quite plain in the dispatches to India and Bengal from London, and may be seen in the Commonwealth Relations Office, King Charles Street, London, S.W.1, through the Superintendent of Records.

Already in 1835 the subject of India's coal supplies for the steamers for inland navigation was under consideration, and this led to the establishment of a Coal Committee in 1836, with Dr. John McClelland (Medical Services) as secretary. The reports of the (Indian) Coal Committee of 1838 and 1846 are well known, but it has so far been practically unknown that John McClelland had conducted correspondence with Charles Lyell and with R. I. Murchison on the question of employing trained geologists in India. The earliest letter I have seen is dated February 10, 1841, from McClelland to Lyell, expressing a desire for a scientific examination of the coalfields of India; the second is dated September 22, 1842, from Murchison to McClelland, in which reference is made to the appointment of Sir H. De la Beche to the post of "geologist of the Ordnance Survey"; and the third is dated May 10, 1843, from Lyell to McClelland, also referring to the establishment of geological surveys in Canada, etc. Then there is a letter, dated August 16, 1843, from the Coal Committee to the Secretary to the Government of Bengal, enclosing the correspondence with Lyell and Murchison, and asking that they might be consulted on this matter of a proper geological survey of India. This was followed by a letter ("Steam", No. 2091, dated September 4, 1843) from the Government of Bengal to the Government of India, with a strong recommendation from His Honour, the Deputy Governor of Bengal. The Government of India, in turn, wrote the following letter:

Home Department
Marine.
No. 13 of 1843.

To
The Hon'ble the Court of Directors
of the East India Company.

Hon'ble Sirs,

We have the honor to submit the accompanying communication from the Government of Bengal respecting the best means of ascertaining, with precision, the hitherto undeveloped resources of the country in the production of coal.

The papers which we transmit will explain to your Hon'ble Court the grounds on which the Hon'ble the Deputy Governor urges the expediency of adopting a system of scientific inquiry both with the view of securing success to such attempts as are made by Govt. and of promoting confidence among private speculators to embark in undertakings for obtaining coal, and in expressing our concurrence in the views and recommendations of His Honor, we beg to request that the selection of the Geologist to be sent out by your Hon'ble Court, may be left to Mr. Murchison, the President of the Geological Society in London.

We have, etc.

Fort William,
The 14th October, 1843.

There then followed a search in England for a suitable geologist to be sent out to India to begin the Geological Survey, and it was not until December 1845 that the following letter was dispatched from London by the Honourable Court of Directors of the East India Company:

Marine Department.
No. 8 of 1845.

Our Governor General of India in Council.

Para. 1. With reference to your letter No. 13 dated the 14th October, 1843, we have to inform you that we have engaged Mr. D. H. Williams to proceed to India for the purpose of making a Geological Survey of those districts in which Coal Fields are situated, with the view of obtaining accurate information respecting the resources possessed by that country for the production of Coal and determining in what manner they may be best turned to account.

2. Mr. Williams has been strongly recommended to us by Sir Henry De la Beche, Director General of the Geological Survey of the United Kingdom under whom Mr. Williams has been for several years employed. Sir Henry informs us that he has surveyed many of the Coalfields in Wales and in the West of England and has executed very valuable maps and sections relating to them. He also represents him as having been in early life engaged in the working of Collieries and as being perfectly qualified to examine the Coal Districts of India, either geologically, or practically as well as to superintend Coal works.

3. These high testimonials leave no doubt of the fitness of Mr. Williams for the important duty which it is proposed to confide to him, and we trust that his labours will have the effect of ensuring success to such mining operations as Government may think proper to undertake and of encouraging private speculators to embark in similar attempts.

4. Mr. Williams left England with the mail of the 20th of the present month (Decr.) and will proceed to Calcutta to place himself under the orders of your Govt. The period of his engagement is limited to five years. He is bound to serve in any part of the East India Company's Territories to which you may direct him.

5. We have agreed to allow him a salary of £800 a year to commence from the date of his arrival in India exclusive of such travelling allowances as you may think reasonable; and we have also granted him £200 in aid for

his outfit and passage to India, and we have guaranteed him the like sum for his return to this country on the expiration of his period of service. He has also been furnished with a set of Instruments such as he is likely to require in India, in order that he may not be prevented from entering immediately on his duties. These instruments, a list of which is transmitted, are similar to those used in the Geological Survey of Great Britain and have been selected by Sir Henry De la Beche.

6. One of the parts of the Deed of Covenant entered into by Mr. Williams is forwarded in the packet for your information and guidance.

We are, etc.

London,
23rd Decr., 1845.

David Hiram Williams arrived in Calcutta on February 4, but did not report for duty, by letter, until next day, February 5, 1846. He was instructed by the Government of India (letter No. 134, Home Dept., Marine, dated February 7, 1846) to place himself at the disposal of the Government of Bengal, and by this Government posted to the Raniganj area. Williams' first field report, from camp at Raniganj, is dated March 31, 1846, and his first progress report, from Spence's Hotel, Calcutta, is dated August 10, 1846. From this report it is evident that Williams had noted: par. 4, the Coal Measures resting directly on the crystalline rocks; par. 7, two groups of coal seams—one with ten seams of coal around Raniganj and the other with fourteen seams of coal near Taldanga near the Barakar River; par. 8, coal near Induhghur (Jharia town), four miles south of the Fitcoory bungalow (ruins near Dhanbad at the Grand Trunk Road—he thus discovered the Jharia coalfield); par. 11, the importance of railway communication; and in a later field report, dated May 3, 1847, he records that the coal near Taldanga was of caking (coking) quality. His last known report is dated December 9, 1847. Williams had planned a series of investigations up the Damodar valley and had secured geological assistants—Mr. Haddon and Mr. Jones—when he contracted jungle fever and died in Hazaribagh in the house of Dr. Callum on November 15, 1848. And, while R. G. Haddon was reporting this sad affair and assuming charge of the Geological Survey at Hazaribagh on November 15, 1848, the body of J. R. Jones, the other assistant, was brought in in a palki. When Williams' death was reported to the Honourable Court of Directors in London in a letter dated January 31, 1849, they replied (letter No. 24, Marine Dept., dated Nov. 28, 1849) to "Our Governor of the Presidency of Fort William in Bengal" complaining of delays in the transmission of reports and as not having news of Williams' death until the autumn of 1849. They record their appreciation of the work done by Williams and propose to publish his reports, and desire McClelland to continue the work, while they add that:

Para. 6. We propose to take immediate steps for securing the services of a suitable successor to Mr. Williams.

and ending "We are your loving friends . . . etc.".

Their next letter (No. 27 of 1850, Marine Dept., Dec. 11) to "Our Governor General of India in Council" includes

"We desire that the earliest attention of the Geological Surveyor (Professor Oldham) whom we have lately appointed, and who will proceed to India this month, be directed to the subject."

The subject was the introduction of railways in various areas such as those to which Williams had directed attention. Then their further letter (Marine Dept., No. 9, March 5, 1851) to "Our Governor of the Presidency of Fort William in Bengal" followed with the counterpart of the covenant entered into by Prof. Oldham,

". . . lately attached to the Irish Branch of the Geological Survey of the United Kingdom whom we have approved to the situation of Geologist, in succession to the late Mr. D. H. Williams . . ."

In the above letter reference is made to their letter No. 24 of November 28, 1849, showing they had chosen "a suitable successor to Mr. Williams" as early as possible. Previous to Oldham's arrival in India (March 5, 1851), McClelland had carried on the duties which Williams had begun, and McClelland submitted the annual "Report of the Geological Survey of India for 1848–49". He had designated himself as 'officiating superintendent', Geological Survey, and had asked (letter dated March 23, 1850) to be relieved of his geological duties. The Department was therefore (letter dated April 1, 1850) temporarily placed under the charge of the deputy surveyor general, Captain Thuiller, R.E., from April 30, 1850, who addressed Mr. R. G. Haddon as assistant geological superintendent. Thus when Prof. T. Oldham arrived, he was in direct touch with the Surveyor General of India, but he was addressed as Geological Surveyor by the Government of India (letter No. 290, Home Dept., of March 21, 1851, from W. Grey, Esq., Under-Secretary) and informed that his services had been placed at the disposal of the Government of Bengal.

No. 189 dated Fort William, the 24th March, 1851.

From

The Secretary to the Government of Bengal

To

Professor T. Oldham.

Sir,

I am directed by the Deputy Governor of Bengal to transmit herewith, for your information, the accompanying copy of a letter from the Under-Secretary to the Government of India, in the Home Department, No. 291 dated the 21st instant, and to request you to proceed to the Sylhet Hills, with the view of prosecuting your researches there until the season will permit of your examining the Valley of the Damoodah. You will address your reports to this office.

2. At the time of the death of Mr. Williams the Establishment of your office was as following:—

Mr. Haddon	Rs350
Mr. Jones	Rs350
A sub. asst. surgeon	Rs100
A writer and interpreter	Rs50
	Rs850

3. Before Dr. McClelland made overcharge of the office the Establishment was as follows:—

Mr. Haddon	Rs350
Mr. Theobald	Rs100
Mr. Gomes	Rs100
	Rs550

The persons still borne upon your establishment are:—

Mr. Haddon	Rs350
Mr. Gomes	Rs100
	Rs450

4. Mr. Haddon and Mr. Gomes are at present on detailed duty at Purnear. But on your intimating to His Honor that their services will be required by you in your

researches in the Sylhet Hills, they will be directed to join at Sylhet, or Cherra forthwith.

5. You are requested to report upon the establishment you may think it necessary to employ.

I have the honor, etc.,
W. Grant,
Secretary to the Govt. of Bengal.

So Prof. Thomas Oldham entered upon his duties as 'geological surveyor' in succession to Mr. D. H. Williams, and when his period of five years expired and his services were secured by his re-engagement for a further period of five years, the Honourable Court of Directors of the East India Company wrote as follows (letter, India Public Dept., No. 16 of March 5, 1856):

". . .

Para. 2. The duties of the Geological Surveyor have been greatly enlarged since Mr. Oldham assumed charge of the office and the establishment which he has to control has been likewise increased owing to the simultaneous prosecution of geological researches in different parts of the country. In consideration, therefore, of the greater labor and responsibility thus attaching to the office, and of the experience which Mr. Oldham has now acquired, we sanction the increase of his salary from the present rate of Rs888 to Rs1,100 per mensem.

Para. 3. We observe with some surprise that the President in Council, as one of the reasons for the increase of Mr. Oldham's salary states that he is paid little more than his assistants. . . .

Para. 4. We take this opportunity of observing that we have not received so regularly as we would desire, reports of the progress of Mr. Oldham's labors. We are apprehensive that the operations of the Department may not have been carried out in a systematic and regular plan. We think it desirable that you should direct your attention to the importance of giving such a character to the work on which Mr. Oldham is engaged, as the only means of rending his employment personally useful and we desire that, in communicating to him his re-engagement you call on him to submit a general scheme in which his future investigations should be carried on, and that when a plan of observations shall have received your sanction, it should be adhered to with as little deviation as possible. We desire a full report of his past labors and any maps he may have made be sent to us.

We are your affectionate friends,

. . ."

I think that the correspondence I have quoted and referred to will make it very evident that India owes a great debt to Dr. John McClelland in initiating a Geological Survey and very much more to the Honourable Court of Directors of the East India Company in providing the personnel and in taking a close interest in the establishment of the department on sound lines, also to the Government of Bengal for the consideration they showed Oldham after the losses suffered by the deaths of Williams, Jones and, soon after, Haddon, from the effects of jungle fever. It may be, and has been, claimed that in the century completed by the Geological Survey of India on February 5, 1946, the whole of India should have been thoroughly examined and mapped. That this could have been done only by having a large staff and elaborate equipment will be seen from the fact that an experienced geologist can map about 500 square miles a season on an average. There have been, roughly, a hundred and twenty geologists with average field service of barely eight years each, so that the area that could have been mapped is about 500,000 square miles, which was barely a third of the Indian Empire. This does not allow for all the detailed work that has been carried out, and in spite of which the Geological Survey of India had examined practically the entire region of India, part of Burma and some adjacent countries, by the end of its centenary year 1946. Instead of a cadre of thirty to forty officers, the department was already in need of three hundred to four hundred officers at the outbreak of war in 1939 to conduct a thorough economic exploration and exploitation on the lines which Mr. David Hiram Williams was to follow, to say nothing of the important branch of water-supply and of questions of engineering geology.

THE BRITISH-KENYA MIOCENE EXPEDITION, 1947

ONE of the discussions at the Pan-African Congress on Prehistory, which took place at Nairobi in January of this year, was concerned with the fossil remains of Miocene apes which have been found in Kenya during recent years. Some of the delegates to the Congress also visited Rusinga Island (near the Kavirondo Gulf of Lake Victoria) where Early Miocene deposits particularly rich in such fossils had been discovered by Dr. L. S. B. Leakey and Dr. D. G. MacInnes of the Coryndon Museum, Nairobi. It was here that Dr. Leakey found in 1942 a practically complete mandible of the extinct anthropoid ape, *Proconsul*, originally described on the basis of portions of the jaws and teeth discovered at Koru by Dr. A. T. Hopwood.

The opportunities provided by the Pan-African Congress served to focus attention on the great importance of these Miocene sites, and as a result a British-Kenya Expedition to Rusinga Island was organised with the aid of a grant of £1,500 from the Royal Society supplemented by a gift of £250 from the Aga Khan. The expedition was led by Dr. Leakey as field director, with the assistance of Dr. MacInnes as palæontologist. A very detailed geological study of the Miocene beds in the island was included in the programme, in order to supplement the preliminary survey carried out by Dr. P. E. Kent in 1934–35 (see *Quart. J. Geol. Soc.*, **100**, 85; 1944). The geological work of the expedition was supervised by Dr. R. Shackleton, of the Imperial College of Science and Technology, London. While the expedition concentrated its attention on Rusinga Island, brief reconnaissance work was also carried out at Mwafangano, Karungu, and on the Uyoma mainland, and a few days were spent on the Miocene site at Songhor. Work was begun on July 10 and the season ended on November 13.

The principal aim of the Expedition was to obtain further remains of the Early Miocene fossil Hominoidea, and to collect the associated fossil fauna and flora in as much detail as possible for a correlated study of the contemporary environment. Full reports of the season's work will take some time to prepare, but it is now possible to say that the results have been eminently satisfactory. More than 1,300 fossils have been collected, and these include about thirty more specimens of the fossil Hominoidea (probably representing five different genera). Among the more important hominoid discoveries are the palate and mandible of a medium-sized ape (regarded provisionally as *Xenopithecus*), a part of a mandible (including the symphysis) of an ape considerably larger than *Proconsul*, the left half of a palate of an infant Hominoid probably referable to *Proconsul*, and some fragments of limb bones.

The last are likely to be particularly instructive since almost nothing is yet known of the skeletal anatomy of Miocene apes or of their limb proportions. There are already available from Miocene deposits in Kenya an almost complete femur of a large primate (presumably *Proconsul*), as well as the shaft of the humerus, a portion of the clavicle, and some tarsal bones. These are in course of examination in the Department of Anatomy at Oxford; if their reference to *Proconsul* is correct, they indicate that, while from the size of its jaws and teeth it is apparent that this fossil ape approximated to the chimpanzee in its general size, it was of much lighter build and a much more active and agile creature. The femur shows a remarkable resemblance to the femur discovered at Eppelsheim at the beginning of the last century and commonly regarded as the remains of a large gibbon-like ape of early Pliocene date. The humerus, also, is similar to that of the Miocene ape, *Dryopithecus fontani*, described by Lartet in 1856.

The associated fossils collected at Rusinga during the expedition include (1) the greater part of the skeleton of a member of the Rhinocerotidæ (allied to, but distinct from, *Aceratherium*), (2) a considerable quantity of remains of a giant Hyracoid, probably referable to *Pliohyrax*, (3) a number of specimens of *Myohyrax*, including a nearly complete skull, (4) very numerous fossil rodent remains, (5) remains of carnivores, anthracotheres, chalicotheres, insectivores and Suidæ, (6) numerous plant remains, (7) numerous fossil fish, (8) a large collection of myriapods and gastropods, and some pelecypods. As soon as this rich collection has been sorted out and catalogued, the several groups which it contains will be allocated to different laboratories for detailed study.

That so much important fossil material has been collected in one season's work is a tribute both to the richness of the Miocene deposits on Rusinga Island and to the energy and skill of the members of the expedition. The discovery of remains of Early Miocene apes of a generalized kind is of particular significance, since they are likely to throw important light on the evolutionary radiations of the whole group of the Hominoidea. For this reason, it is earnestly to be hoped that funds may be forthcoming for another season's work in 1948. There is a reasonable certainty that this would procure still more limb bone material of Miocene Hominoids; but it is also of the greatest importance to search for parts of the skull (in addition to the mandible and maxilla, of which some remains have already been obtained).

Another consideration arises from the outstanding discoveries in Kenya of Dr. Leakey and his associates, not only of primitive fossil apes but also, in the field of archæology, of early remains of man and his palæolithic cultures. This is, that they are now attracting the attention of prehistorians and palæontologists in other countries. It has recently been reported in the Press that a well-equipped expedition from the University of California is now on its way to East Africa to operate on sites such as those discovered by British investigators, and no doubt other expeditions of a similar kind may be expected in the future. I would therefore urge the desirability of establishing in East Africa an Archæological Survey, with an adequate staff and appropriate financial resources, to organise facilities for, and to co-ordinate the activities of, expeditions into this territory. It is much to be hoped that serious attention will be given to this urgent problem.

W. E. Le Gros Clark

PHYSICO-CHEMICAL ASPECTS OF CHEMICAL CARCINOGENS

By WILLIAM ANDERSON

Research Department, Glasgow Royal Cancer Hospital

Experimental

SINCE it was first demonstrated that cancer could be induced by a pure chemical compound, the mode of action of chemical carcinogens has been studied from many different points of view and numerous theories have been evolved from the results of controlled experiments on these substances[1,2]. The following observations present yet another aspect from which the problem may be considered.

A large number of the carcinogenic compounds possess no functional group in their molecules and it is therefore difficult to understand how they can bring about the malignant change in a cell by a purely chemical interaction. Furthermore, although the metabolites of many of the substances are derivatives containing hydroxyl groups, the evidence available indicates that hydroxylation causes a diminution of the carcinogenicity of the parent substances[3]. Consequently, some physicochemical explanation of the mode of action of carcinogens was sought. The idea occurred that carcinogenesis by chemical compounds, in particular the polycyclic aromatic hydrocarbons, might have its origin in a chemiluminescence accompanying the hydroxylation of the substances *in vivo*. This concept seemed feasible since it is known that electromagnetic radiations can bring about malignant transformation, and on the other hand, that oxidation of certain organic compounds is accompanied by the emission of radiation. It is also interesting to note that, simultaneously, other investigators have been led to suggest similar ideas[4,5]. It was decided in the first instance to study the reactions of the polycyclic aromatic hydrocarbons under the influence of hydroxylating agents.

Among the reactions accompanied by chemiluminescence described by Trautz[6] are oxidations with chlorine or bromine water of anthracene, phenanthrene and chrysene dissolved in hot alcohol saturated with potassium hydroxide. These reactions were repeated substituting various other hydrocarbons for anthracene, etc., and in each case chemiluminescence was observed. The compounds tested were: naphthacene, 1 : 2 : 5 : 6-dibenzanthracene, 9 : 10-dimethyl-1 : 2-benzanthracene, 20-methylcholanthrene, and 3 : 4-benzpyrene. The luminescence was momentary, due to the fact that an aqueous medium was added to a solution of the substance in alcohol whereby the material was thrown out of solution. The possibility that the radiation might be a crystalloluminescence was not borne out by control precipitation of the substances with water alone. It is likely that these oxidations were due to the formation of hypochlorite or hypobromite, since chemiluminescence could also be produced by adding an aqueous solution of sodium hypochlorite to the substances dissolved in alcohol.

The next reagent used was perbenzoic acid in chloroform solution, since Eckhardt[7] has described reactions between this hydroxylating agent and several hydrocarbons. Attempts to produce chemiluminescence with 3 : 4-benzpyrene using this reagent proved unsuccessful, but when the perbenzoic acid solution was added to a concentrated solution of 20-methylcholanthrene in chloroform, both solu-

tions being heated to 50° C., a chemiluminescence lasting for three seconds was observed. A similar result was obtained from anthracene, when the luminescence persisted for thirty seconds. When further attempts to produce chemiluminescence with 3 : 4-benzpyrene and 9 : 10-dimethyl-1 : 2-benzanthracene failed, investigations using this reagent were stopped, since the technique was proving costly in hydrocarbons. A possible explanation of the fact that chemiluminescence was observed with anthracene and methylcholanthrene but not with the other hydrocarbons is to be found in Eckhardt's observations, which show that the rates of oxidation of methylcholanthrene and anthracene are the two fastest among those of the numerous compounds which he studied.

Various attempts had been made to produce chemiluminescence using hydrogen peroxide as the oxidizing agent, since it has been suggested[9] that the hydroxylation of hydrocarbons which occurs *in vivo* operates through a hydrogen peroxide system. These attempts were fruitless until the Milas[9] reagents were used, in which the hydrogen peroxide is incorporated in tertiary butyl alcohol and osmium tetroxide is employed as catalyst. With these reagents chemiluminescence has been so far observed with the following hydrocarbons: naphthalene, anthracene, phenanthrene, naphthacene, chrysene, 3 : 4-benzpyrene, 9 : 10-dimethyl-1 : 2-benzanthracene, and 20-methylcholanthrene. To observe these phenomena, the following technique was used: a solution of the substance was made in the (6 per cent) hydrogen peroxide–tertiary butyl alcohol reagent[9] by warming the solvent with the substance and filtering to remove the undissolved excess. To 5 ml. of this solution was added 0·5 ml. of a 0·5 per cent solution of osmium tetroxide in tertiary butyl alcohol. The radiation emitted was observed in a completely dark room, the observer having accustomed himself to the darkness for 15–20 min. prior to making the observation.

In contrast to the reactions with chlorine and bromine water described above, no precipitation occurs here and the chemiluminescence persists for 15–80 sec., so that the reactions are suited to controlled experiment. The experiments with the other reagents had been conducted at raised temperatures (50°–80° C.), but with the Milas reagents radiation has been observed at 37° C. with certain of the hydrocarbons. Several of these oxidations have been studied more fully, and fluorescence spectra of the reaction products together with solubility in alkali indicate the presence of hydroxylated derivatives. Further details of these investigations will be published elsewhere.

The question then arose as to whether the other chemical structures which fall within the classification 'chemical carcinogens' would emit radiation in similar reactions. Metabolism studies show that 2-acetylaminofluorene undergoes hydroxylation in the rat[10]; and although investigations on the azo-dyes suggest that the main metabolic changes which occur are other than hydroxylation, it is perhaps significant that Stevenson, Dobriner and Rhoads[11] isolated *p*-aminophenol from rats dosed with Butter Yellow (*p*-dimethylaminoazobenzene), indicating hydroxylation at some stage in the metabolism. Chemiluminescence was observed using Milas reagents with 2-amino- and 2-acetylaminofluorene, and with Butter Yellow, the N-monomethyl compound, the free base, and *p'*-methyl-N : N-dimethyl-*p*-aminoazobenzene. Tests on other structures showed that radiation is emitted during the reactions with *p*-dimethylaminostilbene and 'styryl 430' [2, (*p*-amino styryl) 6, (*p*-acetylamino benzoylamino)-quinoline methoacetate] and with 3 : 4 : 5 : 6-dibenzcarbazole and its N-methyl and N-ethyl derivatives.

The spectra of the radiations from the reactions have not yet been recorded, since the light is of very low luminosity under the experimental conditions obtaining, but work in this direction is in progress. The radiation so far observed has been in each case a weak green or greenish-white light, with the intensity varying from substance to substance and with the temperature of the reaction mixture. It is possible to grade the intensity of the radiation from a given reaction as weak, medium bright, or bright, but these data have not been recorded here, since obviously they would bear little relation to the intensity of the radiation from a similar reaction occurring *in vivo* where the reactions are of a heterogeneous nature. It might be remarked, however, that a chemiluminescent reaction occurring in the cell would be expected to have a high radiochemical yield, since the molecule when reacted upon will be adsorbed to some chemical structure in the cell (see below), and will therefore be unable to rid itself of excess energy by collision with other molecules to the same extent as occurs in the homogeneous reaction conducted in the test tube.

Theoretical

The observations set out above indicate that similar processes may occur during the reactions on the chemical carcinogens which take place *in vivo*. If we consider the emission of radiation during reaction to be one of the factors concerned in chemical carcinogenesis, and, indeed, to be the factor which originates the breakdown of the stable economy of a cell or a group of cells, we can evolve a theory which allows some degree of correlation of the great mass of scientific data, chemical and biological, on the subject, and which at least lends itself to further deduction and experimental test.

The experiments in chemiluminescence show that here is a phenomenon associated with the oxidation of all the major groups of chemical carcinogens, and the concept that this property initiates the malignant change, at once, finds a common factor for the different series of compounds, and explains, in part, the failure of attempts to correlate chemical structure and carcinogenic action. Furthermore, a theory on this basis provides a link between the chemical carcinogens and the physical carcinogens. That this is a satisfactory link is suggested by the fact that, histologically, tumours induced by chemical and physical carcinogens are remarkably alike, while in some cases they can be distinguished from virus-induced tumours[12,13,14]. The mechanism visualized is as follows: molecules of the chemical carcinogen are supplied continuously over a time period to a localized group of cells by painting the carcinogen on a given area of the skin, or by subcutaneous injection of an oily solution, or by feeding. These molecules pass into the framework of the cells and at some particular point in the cell they come into contact with a hydroxylating system. In the ensuing reaction radiation is emitted and some molecules are hydroxylated. The radiation which is liberated inside the cell is then absorbed by some other molecule of the cell structure, and presuming the radiation to be in the visible or near ultra-violet regions of the

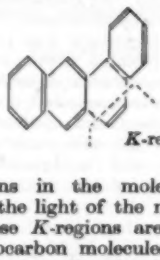
spectrum it will cause electronic transitions in this molecule. The result of these transitions will depend on several factors, such as the frequencies of the light absorbed, the strength of the particular chemical bonds affected, and the means which the energized molecule has at its disposal to lose the extra energy; but, at any rate, certain chemical bonds will be momentarily weakened or made more reactive. It may be that, at a certain juncture during the life of the cell, a state of affairs obtains in which the cell structure is under greater strain than normally, and activation of particular chemical bonds by radiation at this time will be effective in producing some structural change; such a juncture is reached during mitosis.

Reference should be made at this point to the interesting article of Haddow[2] on the energetics of the malignant transformation, where he shows that evidence drawn from different sources suggests that the change from normal to malignant cell may be related to some change of energy-level in a certain part of the cell.

In the different studies designed to discover the mode of action of chemical carcinogens an attempt has always been made to relate the carcinogenic potency of the different substances and the extent to which they exhibit the particular property under consideration. The difficulty of making a significant comparison in this way becomes apparent when we come to realize that many factors contribute to the meaning of the apparently straightforward phrase 'carcinogenic potency'. In furtherance of the theory of carcinogenic action proposed above, certain facts which might explain the so-called carcinogenic potency of a substance will be considered.

A point of considerable interest which has emerged from the careful metabolism studies on the hydrocarbons is that the hydroxy-metabolites have the hydroxyl radicals in positions which are not the most chemically reactive ones[15]. The inference from this is that in the reactions which occur *in vivo* the most reactive positions are in some way blocked, and the suggestion has been made that prior to the reaction the molecule is adsorbed to some part of the enzyme system which effects the hydroxylation[16]. This idea is much favoured by the observation that, while the mechanism of enzyme catalysis is not fully understood, one feature which is agreed upon is the formation of an enzyme-substrate complex[17]. This interpretation is given a new significance when it is considered in conjunction with the recent work of the Pullmans[18], who claim that a relationship exists between the carcinogenic activity of a substance and the value of the electron density about a specific bond in the molecule of the substance. The attempt to obtain evidence that such a relationship exists was initiated by Schmidt[19], who also suggested an interpretation[20]. Schmidt's idea is that hydrocarbon molecules possess a region where the electron density is higher than at other parts of the molecule, and that if this density exceeds a certain value the substance is a carcinogen. Carcinogenesis is then supposed due to an interaction between the region of high electron density and some protein molecule whereby a quantal change in the protein molecule is facilitated. The mathematical treatment has been modified and placed on a firmer basis by the Pullmans, while Schmidt's discussion on the significance of the results has been revised by Daudel[21].

The results of the Pullmans' work are capable of an alternative explanation which is supported by chemical facts. A high concentration of electrons about a specific region in a molecule will confer on that region the property of greater reactivity; thus the *K*-regions to which the Pullmans refer in the hydrocarbons are the most



K-region

reactive positions in the molecules. When we consider this in the light of the metabolism studies, we see that these *K*-regions are the positions by which the hydrocarbon molecules will be attached to the enzyme system, thus leaving the positions of secondary reactivity to be hydroxylated. Furthermore, evidence in support of the likelihood of this type of complex formation is offered by the studies of Weiss on the complexes formed between the hydrocarbons and quinones (or polynitro compounds)[22] and on the salts of these hydrocarbons with different acids (perchlorate, sulphate, pyrophosphate)[22]. In both types of structure there is an electron transfer from the hydrocarbon to the other component of the complex and the formation of a linkage of an ionic nature. Such an electron transfer would originate at the *K*-region of the hydrocarbon molecule. The suggestion is therefore made that the significance of the presence of regions of high electron density in the molecules of polycyclic aromatic hydrocarbons is that it provides them with a means whereby they can attach themselves to a particular enzyme system within a given cell. Since the calculations of the Pullmans have been extended to include other carcinogenic structures, it is possible that a similar explanation might hold in these cases also. If, indeed, a relationship exists between the electron density values at these regions of the molecules and their carcinogenic potency, this would indicate that the ease with which the molecules of a substance can be adsorbed by a particular enzyme system plays a major part in deciding the carcinogenic potency of a compound, and whether it will be a carcinogen at all.

Let us now consider the idea that chemiluminescence is the direct factor causing the malignant change in conjunction with the idea that the ease with which a substance can be adsorbed to a particular enzyme system is the direct factor deciding the relative potency of the substance as a carcinogen. We are then in a position to offer a simple explanation of three interesting facts: (1) that certain chemical substances are carcinogenic, (2) that there are differences in the carcinogenic potency of these substances, (3) that compounds closely related in chemical structure to the carcinogens have no carcinogenic activity. We may suggest that a chemical compound will be carcinogenic if, during its metabolism, sufficient energy in the form of radiation of a particular set of wave-lengths is liberated. The potency of a carcinogen will be dependent on the ease with which it can attach itself to a particular enzyme system; the more easily the molecule is adsorbed, the more readily will the reaction proceed and energy be liberated, thereby presenting a greater probability for structural change in the cell. When the ease of adsorption and consequently the rate of metabolism falls below a certain

value, the risk presented to the cell structure is too small and the substance is a non-carcinogen.

The term 'carcinogenic potency' must be interpreted cautiously. Indeed, the relative potency of a carcinogen varies from one cell type to another, and we must qualify the above ideas to cover this point. If the potency of a carcinogen is dependent on the amount of energy liberated during a given time period, it will be directly controlled by the rate of the metabolism of the substance. Other factors which will influence the rate of metabolism of a compound besides ease of adsorption to the enzyme system responsible for the reaction are the ease with which the molecules of the compound can reach that system, and the ease with which the reactants can be desorbed and removed from it to allow for further reaction. These properties will depend on the precise spatial configuration of the molecule of the carcinogen and of the cell structure, and on the intimate interactions between these. It follows that differences in cell structure will affect the rate of metabolism of the carcinogen; it is inferred that they will thus affect the potency of the carcinogen.

[1] Cook, J. W., and Kennaway, E. L., *Amer. J. Cancer*, **39**, 398 (1940).
[2] Haddow, A., *Brit. Med. Bull.*, **4**, 331 (1947).
[3] Berenblum, I., and Schoental, R., *Cancer Res.*, **3**, 148 (1943).
[4] Figge, F. H., *Cancer Res.*, **6**, 498 (1946).
[5] Weigert, F., 23rd Ann. Rep., Brit. Emp. Can. Campaign, 77 (1946).
[6] Trautz, M., *Z. physikal. Chem.*, **53**, 1 (1905).
[7] Eckhardt, H. J., *Ber. deutsch. chem. Gesellsch.*, **73**, 13 (1940 B.).
[8] Fieser, L. F., "Production of Cancer by Polynuclear Hydrocarbons". (Univ. of Pennsylvania Bicentennial Conference, Philadelphia, 1941.)
[9] Milas, N. A., and Sussman, S., *J. Amer. Chem. Soc.*, **58**, 1302 (1936).
[10] Bielschowsky, F., *Biochem. J.*, **39**, 287 (1945).
[11] Stevenson, E. S., Dobriner, K., and Rhoads, C. P., *Cancer Res.*, **5**, 235 (1942).
[12] Peacock, P. R., *Glasgow Med. J.*, **127**, 157 (1937).
[13] Peacock, P. R., *Cancer Res.*, **6**, 311 (1946).
[14] Rous, P., *J. Exp. Med.*, **69**, 399 (1939).
[15] Berenblum, I., and Schoental, R., *Cancer Res.*, **6**, 703 (1946).
[16] Cook, J. W., *Ann. Rep. Chem. Soc.*, **39**, 155 (1942).
[17] Höber, R., "Physical Chemistry of Cells and Tissues", 391 (Churchill, 1945).
[18] Pullman, A., and Pullman, B., *Rev. Sci., Paris*, **84**, 145 (1946).
[19] Schmidt, O., *Z. physikal. Chem.*, (B), **42**, 83 (1939).
[20] Schmidt, O., *Z. physikal. Chem.*, (B), **44**, 194 (1939).
[21] Daudel, R., *Rev. Sci., Paris*, **84**, 37 (1946).
[22] Weiss, J., *J. Chem. Soc.*, 245 (1942).
[23] Weiss, J., *Nature*, **147**, 512 (1942).

OBITUARIES

Prof. P. Lenard

PHILIPP LENARD was born at Pozsony (Pressburg) in Hungary on June 7, 1862. In the town of his birth he was fortunate in finding an excellent teacher of physics, Virgil Klatt, in conjunction with whom he later carried out his first work on phosphorescence. As a young man he studied physics at the Universities of Budapest, Vienna, Berlin and Heidelberg under, among other famous teachers, Bunsen, Helmholtz, Königsberger and Quincke, obtaining a thorough all-round knowledge of theoretical as well as experimental physics. His first senior appointment was, in fact, as professor of theoretical physics in Heidelberg in 1896; two years later he was made professor of experimental physics at Kiel, where he remained until recalled to Heidelberg in 1907 to be Quincke's successor as professor of experimental physics. In 1909 he was made in addition director of the newly founded Radiologisches Institut, which was more or less amalgamated with the physics research laboratory of the University. He remained as professor at Heidelberg until his retirement in 1931. News has recently reached England of his death on May 20, 1947.

Lenard's first paper was on the vibrations of water drops, recalling the fact that Bohr's first paper was the determination of the surface tension of water by jet vibration, and his researches covered a wide field, but he is best known by his pioneering work on cathode rays and electron physics in general. Hertz, whose assistant he was at the time, had shown in 1892 that cathode rays would pass through thin metal foils. Lenard followed this up, and in 1893 succeeded in making a cathode ray tube with a very thin vacuum-tight metal window, with which he obtained outside the generating tube a beam of cathode rays which could either be passed through gases or solids, or be tested in the highest attainable vacuum. His systematic experiments on the passage of cathode rays through matter showed that the absorption was roughly proportional to the density and, further, that it decreased very rapidly with increasing velocity of the rays. He proved that cathode particles of high energy could pass right through atoms, and explained this by his theory that the positive and negative changes within the atom were grouped in tight couplets, which he called 'dynamids'. This theory was, of course, shown to be untenable by Rutherford, but nevertheless Lenard was the first to insist on the charges being grouped so that the space within the atom was relatively empty and penetrable by fast particles. Lenard was the first to prove that the negative electricity released in the photo-electric effect consisted of electrons, although J. J. Thomson's independent demonstration appeared very soon after. For his work on cathode rays he was awarded the Nobel Prize for Physics in 1905.

Lenard was also a pioneer in the field of ionization potential, being the first to show that an electron must possess a certain minimum energy before it can produce ionization. His earliest estimate of 11 volts as the ionization potential of hydrogen, nitrogen and oxygen was a very good one for the rough methods then available. His original experimental disposition was the pattern for many subsequent methods. Another field in which Lenard carried out fundamental work was phosphorescence. First of all in conjunction with Klatt, and later alone and in conjunction with Wilhelm Hausser and others, he studied in great detail the preparation, excitation and emission of light with the alkaline earth and zinc phosphors. In these investigations it was established that the light emission was due to small traces of foreign metal in the sulphide, for example, calcium sulphide, which constituted, with a flux, the bulk of the material, and Lenard put forward the theory that the excitation involved the removal of an electron from the metal atom, which electron was held in certain ways in the surroundings until subsequently released, when its return to the metal atom caused light emission. This is the general basis of the theory now accepted. He also established experimentally that the process was of a quantum nature and, in general, laid the basis for the precise quantitative study of the subject.

He carried out important work in many other directions. He worked out the method of measuring magnetic fields by the bismuth spiral, his work on the electrification caused by splashing was important in its time and he was actively concerned with the electrical properties of flames. Mention must also be

made of the important body of work which he carried out, mainly in conjunction with Ramsauer, on the ionization produced by ultra-violet light, which is not known as widely as it might be.

Lenard was personally a difficult man, whose character contained many contradictions. An intimate friend of mine who knew him well once wrote to me: "Was Lenard betrifft, so ist er so klug und so dumm wie immer". He was profoundly disappointed not to have discovered the Röntgen rays, which he had almost under his hand and would have in all probability found within a year or so of the actual date if Röntgen had not anticipated him. He never used Röntgen's name in referring to the rays. He took as a personal affront any inadequate acknowledgment of his work and was incapable of any generosity or even justice towards anyone who, in his opinion, had failed to appreciate any part of his services to science. Although he owed much of his success to Jews, for example, Hertz and Könisberger, and at one time freely acknowledged the debt, he became a bitter anti-Semite and even treated Einstein as not far from an impostor. He refused to fly the Institute flag at half-mast when Rathenau was murdered and was with difficulty saved from popular indignation. He became a whole-hearted supporter of the Nazi regime and of the 'German physics' movement: in fact, he wrote a book called "Deutsche Physik". Yet he had a kindly side to his nature and was often a pathetic rather than a menacing figure. He possibly felt a deep personal need of friendship which he was unable to win or, if he could win it, to retain. His lack of trust in others, his failure to awaken the self-reliance or sympathy of those working under him were the cause that he did not found a great school of physics. It has been suggested, probably correctly, that the clue to his character was that he was a weak personality that sought to protect itself by a hard shell.

As an experimental physicist, Lenard was certainly one of the greatest figures of his time. His work on the physics of the electron was distinguished by a masterly experimental technique and his discoveries had a profound influence on the course of physics, in particular his work on the release of electrons by electron impact and by light. Yet he seemed fated never to achieve supreme greatness. He missed the discovery of Röntgen rays; he came near to the discovery of the true structure of the atom, but just went astray, and his work on light emission was the first to indicate the important part which the release and return of the electron played, but left to Bohr the great advance. He was a whole-hearted enthusiast for experimental physics, whose appreciation of the great men of science of the past times was generous and informed, as can be seen from his book "Grosse Naturforscher" (translated into English under the title "Great Men of Science"). He was a dark genius beclouded by strong personal fears, doubts and envies, but undoubtedly a genius and one who has left an abiding impression in physics.

E. N. DA C. ANDRADE

WE regret to announce the following deaths:

Dr. C. C. Hurst, of Cambridge, known for his pioneer work in genetics, on December 17, aged seventy-seven.

Sir Bernard Spilsbury, honorary pathologist since 1934 to the Home Office, on December 17, aged seventy.

Mr. Benjamin Talbot, Bessemer medallist in 1908 and president in 1928 of the Iron and Steel Institute, on December 16, aged eighty-three.

NEWS and VIEWS

Committee on Industrial Productivity

IN answer to questions in the House of Commons on plans for further development of scientific research to assist industrial production, Mr. H. Morrison, Lord President of the Council, stated on December 18 that he is advised that, while a major contribution to industrial productivity cannot be expected in the short run from current research in the natural sciences, there are considerable possibilities of increased returns, first from the more widespread application of research already carried out in the natural sciences and technology, and, secondly, from current research in the social science field. He had, therefore, decided, in consultation with the Chancellor of the Exchequer, to supplement the work of the Advisory Council on Scientific Policy by setting up a new Committee on Industrial Productivity. The terms of reference of the Committee are: "To advise the Lord President of the Council and the Chancellor of the Exchequer on the form and scale of research effort in the natural and social sciences which will best assist an early increase in industrial productivity, and further to advise on the manner in which the results of such research can best be applied". The main work of the Committee, of which Sir Henry Tizard will be chairman, will be conducted through a number of panels constituted from time to time to investigate and report on various aspects of the problem. In the first instance panels are being set up to deal with technology and operational research under the chairmanship of Sir William Stanier; import substitution, under Prof. S. Zuckerman; the human factors affecting industrial productivity, under the chairmanship of Sir George Schuster; and technical information services, under the chairmanship of Dr. Alexander King. The remaining members of the Committee will include one or more employers (chosen in consultation with the F.B.I. and the B.E.C.); one or more trades union members (chosen in consultation with the T.U.C.); Sir Edward Appleton; Sir Claude Gibb; Mr. Hugh Weeks; Mr. Robert Hall; Mr. E. M. Nicholson; Mr. G. B. Blaker; secretary, Mr. E. D. T. Jourdain.

'Flying Wing' Aircraft

INFORMATION has just been released that an aircraft of the 'flying wing' type has completed its trial flights at Bitteswell Aerodrome, near Rugby. It is known as the *A.W.*52, built by Messrs. Armstrong Whitworth to the designs of Mr. J. Lloyd, their chief designer. The aircraft embodies two fundamental principles that have developed out of the general progress of aerodynamic research. The abolition of the tail has long been an ideal to some schools of thought. It sets up considerable drag and does not contribute to the performance of the machine, but is necessary for

control purposes with the more conventional lay-out of the various aerodynamic surfaces. The Westland-Hill 'Pterodactyl' demonstrated the possibilities of tailless aircraft many years ago. The suppression of as much of the external body as practical requirements of accommodation allow is another obvious trend for the same reason.

Experimental work on high-speed flight has made it evident that swept-back wings are going to be inevitable at transonic and supersonic flight speeds. Again the vital factor appears to be that of control with the less conventional lay-out of the wing surfaces. The *A.W.52* follows a series of experiments carried out with gliders, and appears to be satisfactory in this respect. Further progress towards even higher speeds should be possible now that greater powers can be envisaged using the internal combustion turbine in conjunction with jet propulsion, and this or similar machines will undoubtedly be used in this way.

Falkland Islands Dependencies Survey

THE Falkland Islands Dependencies Survey has recently acquired a vessel to enable it to carry out its expanding field of work in the Antarctic. The ship, which was named *John Biscoe* by Mrs. Creech Jones, wife of the Secretary of State for the Colonies, on December 16, has a double-skin wooden hull and a displacement of 1,015 tons; she is driven by Diesel-electric power. She carries a crew of twenty-five, and is to take a relief party in the summer to replace about half the party at present occupying the seven observation stations maintained by the Survey in the Antarctic. The new party will carry out geological, meteorological and zoological investigations, and is under the leadership of Dr. V. E. Fuchs, a geologist who has had experience of exploration in East Greenland and in East Africa. The *John Briscoe* has been named in memory of the discoverer of the southern part of Graham Land. The Falkland Islands Dependencies Survey (see *Nature*, March 22, p. 388) was set up under the control of the Colonial Office after the War to take over from the Admiralty the meteorological stations established in the Antarctic in 1943.

Royal Society: Research Appointments

THE Royal Society has appointed Dr. K. Bailey, of the Biochemical Laboratory, Cambridge, to an Alan Johnston, Lawrence and Moseley Research Fellowship for research on the problems of human and animal health and diseases and the biological field related thereto. Dr. Bailey will work at the Biochemical Laboratory in Cambridge on the proteins of muscle fibril. The Royal Society has also appointed Dr. G. S. Dawes, of Worcester College, Oxford, to a Foulerton Research Fellowship in Medicine. Dr. Dawes will work at the Department of Pharmacology, Oxford, on the left ventricle reflex.

Edward Rigby (1747–1821)

EDWARD RIGBY, who was born at Chowbent in Lancashire two hundred years ago on December 27, was a versatile man, distinguished alike in obstetrics, agriculture and civic administration. In medical literature his "Essay on the Uterine Hæmorrhage" (1775) is regarded as a classic, for it clearly differentiated between premature separation of the normal placenta (accidental hæmorrhage) and placenta prævia (unavoidable hæmorrhage). It was translated into French and German, and up to the hour of his last illness the author was employed in the preparation of a sixth edition. A scientific agriculturist, Rigby experimented on his own farm at Framingham Earl, near Norwich, and the reputation of his writings on that subject led to his election as honorary member of the Philadelphia Society. In 1805 he became mayor of Norwich, and he is said to have "supported the chair with dignity and hospitality". He established in 1786 the Norfolk Benevolent Medical Society for the relief of the widows and orphans of medical men, introduced the flying shuttle to the manufacturers in Norwich, and in 1812 started vaccination in the city. Simple, placid and benevolent, Rigby was a delightful companion and a good conversationalist. He died on October 27, 1821, in his seventy-fourth year. His "Letters from France" constitute an animated and fascinating record of the stirring days at the outbreak of the French Revolution, besides forming a useful survey of the country's agriculture at the time. Rigby's son Edward (1804–60) was a well-known London obstetrician.

New Nuclear Reactions

"NUCLEAR Transformations in the new High Energy Ranges" is the title of an address delivered by Prof. Glenn T. Seaborg, of the University of California, at a meeting of the American Chemical Society in New York on September 15 (see *Chemical and Engineering News*, 25, 2819; 1947). The great progress made recently in constructing new powerful machines for the acceleration of charged particles affords the possibility of bombarding targets with electrons of 100 MeV., and with deuterons and helium ions of 200 and 400 MeV. respectively. Entirely new nuclear reactions have been observed; for example, $^{75}_{33}As$, bombarded with 400 MeV. helium ions, produces among other atomic species the well-known $^{36}_{17}Cl$. This means that the arsenic atom has lost no less than 16 protons and 21 neutrons, not counting the two protons and two neutrons shot into it at the start of the reaction. To distinguish such transmutations from the ordinary nuclear reactions in which only one or two particles are ejected, and from fission reactions in which the nucleus is broken into two fragments, the term 'splintering reaction' is proposed. Further, under such strong bombardment fission can be enforced in such elements as bismuth, lead and thallium, and even in an element so low in the periodic table as tantalum; but as the secondary particles emitted have a much smaller energy, there is no possibility of inducing a chain reaction such as in uranium. Many radioactive isotopes so far unknown have been found as a result of these new nuclear reactions, which have still further widened the field radiochemists are called upon to explore; the splintering often leads to neutron-deficient isotopes and increases therefore in a welcome manner the comparatively small number of positron emitters among the artificial radio-elements. Some of them may find application as tracers. Still more important may be the physical aspects of this extension of the energy range; the creation from energy of mesotrons and, if the region of billions of electron-volts can be reached, also of neutrons and protons, seems now a possibility.

Special Libraries in Australia

RAPID growth in the appreciation of the value of accurate and up-to-date information in science and technology was marked during the last decade. This

resulted in recognition that the work of librarians in special libraries and of information officers has a character of its own, and in the consequent formation of associations providing a platform for the discussion of common interests. The Association of Special Libraries and Information Services (A.S.L.I.S.) in Melbourne, Australia, is one of these, and it is to be congratulated on the launching of its journal, *Information*, in June 1947. This first issue contains, besides introductory matter, the first part of an article on documentary reproduction, another on patents in information, glimpses of the B. J. Ball, Ltd., library in Melbourne, and the constitution of the Association. An exchange corner for periodicals, abstracts of relevant literature, and questions and answers on the universal decimal classification are other features which will appeal to members. Although printing difficulties may make the appearance of the journal infrequent at first, it has made a good start. It should, perhaps, be mentioned that the Australian organisation A.S.L.I.S. is quite independent of the Association of Special Libraries and Information Bureaux (A.S.L.I.B.) in Great Britain, though the two are in very cordial relationship.

Indians of the South-Eastern United States

THE historical Indian tribes of the south-eastern United States are diverse in language, but they share so many important characteristics that they form a cultural province. This is illustrated by the arrangement of Mr. Swanton's massive work (Smithsonian Institution: Bur. of Amer. Ethnology, Bull. 137. Pp. xiii+943+107 pl. Washington, D.C.: Gov. Print. Office. 2.75 dollars), in which a brief, purely historical sketch of each individual tribe is given first, leaving the material culture to be dealt with as a whole. The book may be considered as supplementing and bringing up to date the relevant parts of the great two-volume Handbook of American Indians North of Mexico, edited by F. W. Hodge, published as Bulletin 30 of the same series in 1907 and 1910. The author regards it principally as a collection of source materials, and as such he hopes that it will be useful to future students. This hope should be fully realized, and in addition it will be a most useful book of reference. Some of the plates are not well chosen. It is difficult, for example, to see what is to be learnt from the rather distant group, in Edwardian dress, in front of a modern wooden building, labelled "Ladies Relief Society of the Church of Jesus Christ of Latter Day Saints, Catawba Reservation" (Plate 5). The space occupied by this and some others might have been better employed in illustrating objects of material culture.

Bibliography of Seismology

VOLUME 13, No. 19 of the *Bibliography of Seismology*, published by Dr. Ernest A. Hodgson from the Dominion Observatory, Ottawa, Canada, covers items 6,047–6,142 and carries the bibliography to the end of June 1946. This valuable publication, which deals with pure and applied seismology and subjects throwing light on them, covers in this issue world books, papers and works published on the Continent of Europe during the years 1939–45, together with a small list of patents. There are fourteen items from Holland including an obituary notice of Dr. G. Van Dijk (1877–1940), L. P. G. Koning on deep focus earthquakes (items 6,094–95), F. H. van Rummelen and S. W. Visser on Dutch earthquakes, and a text-book on seismology by S. W. Visser (items 6,135, 6,137–38). The items from Finland, five in number, consist of four papers on isostasy and one on pure seismology, namely, Eijo Vesanen's "Ueber die typenanalytische Auswertung der Seismogramme" (*Ann. Acad. Sci. Fennicæ*, A, 3, Geologica-Geographica, 5, pp. 244. Helsinki, 1942). Three items from France include obituary notices of Dr. Edmund Rothé and Dan Barfod La Cour and an article on French geophysics during the War. Item 6,107 contains references to a most important topical seismological question, namely, the use of the seismograph as a meteorological instrument; by means of microseisms tropical storm and cyclone centres at sea can be located and tracked. There is a list of the seismological items published in *Nature*.

An Unusual Asteroid

AN asteroid discovered in August by C. A. Wirtanen, Lick Observatory, is described in *Sky and Telescope* of October. Its magnitude at the time of discovery was 15, and it is estimated that its diameter is only a few miles. Its orbit is nearly circular and lies close to that of Mars, approaching the orbit of Mars to within 6 million miles; but its least distance from the earth is about 50 million miles. Its period is nearly 2·3 years—the shortest period of any asteroid with the exception of Eros.

Institution of Mining and Metallurgy: Officers for 1948–49

THE following have been elected officers of the Institution of Mining and Metallurgy for the session 1948–49: *President*, Mr. Sydney E. Taylor; *Hon. Treasurer*, Mr. Robert Annan; *Vice-Presidents*, Mr. G. Keith Allen, Mr. Donald Gill, Mr. V. Harbord, Colonel L. C. Hill, Sir Arthur Smout and Brig. R. S. G. Stokes. Mr. S. E. Taylor, the new president, was educated at Cambridge, where he read engineering science. He joined the technical staff of Messrs. John Taylor and Sons, managers and consulting engineers to mines in many countries, in October 1922, and became senior partner in 1941. In 1924 he visited Brazil, and since 1923 has paid numerous visits to India in connexion with the management of the mines on the Kolar Gold Field in Mysore State, and the re-opening of the Hutti gold mine in Hyderabad State.

Announcements

THE Royal Society is compiling a list of British scientific societies, together with the names of their officers, etc. As it is desired to make this as comprehensive as possible, it would be appreciated if any secretaries of such societies, who have not yet received a questionnaire from the Royal Society, would inform the Assistant Secretary, Royal Society, Burlington House, London, W.1.

TRINITY COLLEGE, Cambridge, offers research studentships and Dominion and Colonial exhibitions every year, and elections will take place in July 1948. Research studentships are open to graduates of universities other than Cambridge who are not more than twenty-six years of age. The exhibitions are open to undergraduates as well as those wishing to undertake research. Applications, to be sent through the head of the candidate's university, must reach the Senior Tutor, Trinity College, Cambridge, not later than May 1, 1948.

LETTERS TO THE EDITORS

The Editors do not hold themselves responsible for opinions expressed by their correspondents. No notice is taken of anonymous communications

Molecular Weight of α-Amylase

PROF. KURT H. MEYER and his co-workers[1,2] have recently succeeded in the crystallization of α-amylase from pig's pancreas. The molecular weight of the substance, kindly supplied by Prof. Meyer, has been determined from sedimentation and diffusion experiments. The dried substance was dissolved in a buffer solution of *p*H 8·4, with a molarity of 0·2 in sodium chloride, 0·02 in boric acid and 0·005 in borate.

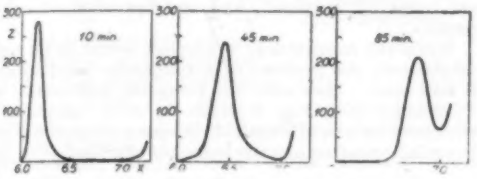


Fig. 1. SEDIMENTATION CURVES FOR α-AMYLASE

Ultracentrifuge measurement. The sedimentation diagrams of Fig. 1 were obtained by centrifuging at 65,000 r.p.m. The diagrams show one definite peak, characteristic of a uniform substance[3]. The concentrations of amylase used are shown in the table.

Conc. of amylase %	s_{20} (in *S* units)
0·09	4·63
0·18	4·32
0·22	4·44
0·26	4·50
0·30	4·38
0·66	4·64

The value of the sedimentation constant is evidently independent of the concentration of amylase. The average value is 4·50 *S*.

Diffusion measurement. The determination of the diffusion constant was carried out according to Lamm's scale method[4,5] in a diffusion cell of steel[6]. The resulting curves are shown in Fig. 2.

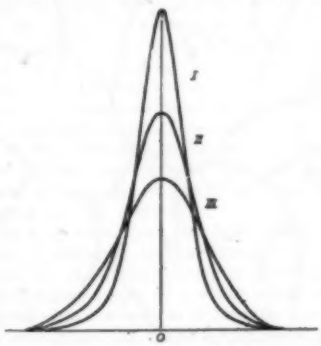


Fig. 2. DIFFUSION CURVES
I, after 20,100 sec.; II, after 42,700 sec., and III, after 80,900 sec.

The diffusion constant was calculated by two independent methods. Values of $D_A = 7{\cdot}89 \times 10^{-7}$ and $D_{80} = 8{\cdot}19 \times 10^{-7}$ were obtained. Hence, the agreement between the two methods of calculation is good. The average value of $D = 8{\cdot}05 \times 10^{-7}$ is used in the following.

The molecular weight was calculated according to Svedberg's formula: $M = \frac{R\,T\,s_{20}}{D(1 - V\rho)}$, where R is gas constant, T is absolute temperature, s_{20} is sedimentation constant, D is diffusion constant, V is partial specific volume of the solute, ρ is density of the solvent.

The value of V was $0{\cdot}70 \pm 0{\cdot}02$, kindly determined by Prof. C. Drucker. Using these values, the molecular weight of this α-amylase is 45,000.

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Sept. 2.

[1] Meyer, K. H., Fischer, E. H., and Bernfeld, B., *Experientia*, **2**, 362 (1946); *Helv. Chim. Acta*, **30**, 64 (1947).
[2] Fischer, E. H., "La purification et l'isolement de l'α-amylase de pancréase", Thèse No. 1094 (Genève, 1947).
[3] Svedberg, T., and Pedersen, K. O., "The Ultracentrifuge" (Oxford, 1940).
[4] Lamm, O., *Nova Acta Reg. Soc. Sci. Upsal.*, Ser. iv, **10**, No. 6 (1937).
[5] Lamm, O., and Polson, A., *Biochem. J.*, **30**, 528 (1936).
[6] Claesson, S., *Nature*, **158**, 834 (1946).

Structure of Starch: Mode of Attachment of the Side-chains in Amylopectin

ESTIMATIONS of the proportion of glucose residues present as end-groups in the amylopectin component of starch have shown that some 5 per cent of the glucose residues occur as terminal groups. Since amylopectin has a high molecular weight, of the order of 10^6, a branched-chain structure must be present in the molecule. The main features of the structure are indicated diagrammatically in Fig. 1[1], where (*A*), (*B*), (*C*), etc., are chains containing on

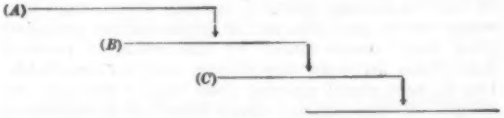

Fig. 1

the average 20–24 1 : 4-linked α-gluco-pyranose residues, each chain being united through C_1 of its first glucose residue to one of the glucose residues in another chain. The location in the chain of this glucose residue is not known, but it is clear that it must have three of its hydroxyl groups, namely, those at C_1, C_4 and at the carbon atom involved in the junction, occupied by linkages to other glucose residues. Two hydroxy groups remain free, and it follows that an investigation of the dimethyl glucose obtained on hydrolysis of a completely methylated amylopectin should enable the mode of linkage to be ascertained. Experiments on these lines showed that 2 : 3 dimethyl glucose was present in the hydrolysis products[2], and it was inferred that some at least of the side-chain linkages were through C_6 of the glucose residues concerned. Uncertainty is caused, however, by the experimental difficulty encountered in the complete methylation of amylopectin and by the fact that trimethyl glucose tends to undergo some demethylation in contact with methanolic hydrogen chloride. An effort to minimize these difficulties was made by using for the methanolysis a fully methylated but highly disaggregated sample of starch of low mole-

cular weight[3]. Again, however, 2 : 3-dimethyl glucose predominated in the dimethyl hexose fraction.

An alternative method of approach was clearly desirable, and this we have endeavoured to provide by a development of the periodic acid method of oxidation of glycols[4]. Inspection of Fig. 2 will show that when the side-chain is attached to C_4 there

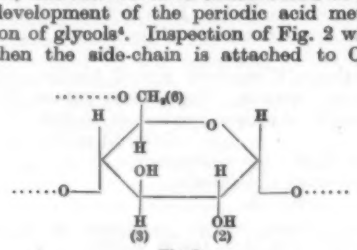

Fig. 2

remain in the glucose residue two contiguous OH groups at C_2 and C_3. Oxidation with periodic acid will therefore disrupt the bond between C_2 and C_3, and after subsequent hydrolysis the sugar residue involved in the branching will no longer be recoverable as glucose. On the other hand, if the side-chain junction is at C_3 or C_2, no α-glycol groupings remain and there will be no attack by periodic acid. In these circumstances the glucose residue remains intact and will be recoverable as glucose after hydrolysis. It follows that oxidation by periodic acid followed by hydrolysis and examination of the products should give information about the mode of linking, provided methods are available for the detection and estimation of small quantities of glucose in admixture with aldehydic and acidic products. These were found in the paper chromatogram method of Partridge[5] and in the recently described quantitative development of this technique[6].

This procedure has now been applied to starches of various origins, namely, potato, acorn, sago and waxy maize, and also to the limit dextrin obtained from waxy maize starch by the action of purified β-amylase. In every case glucose was just detectable, but in such small amount (less than 1 per cent by weight of the starch used) that its quantitative significance is uncertain. On the other hand, the point of special interest which emerges is that it now becomes clear that, in all these amylopectins, by far the greater proportion of the side-chain linkages (more than 75 per cent) are of the 1 : 6 variety, and at most only a small proportion of them can involve either C_2 or C_3, a result which is of particular interest in connexion with theories of the enzymic synthesis of amylose and amylopectin from glucose 1-phosphate. The method is obviously applicable to other problems in polysaccharide chemistry, and it has recently been utilized by Mr. A. E. Flood in this laboratory in a demonstration of the branched-chain structure of several plant gums, including xylan.

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[1] Haworth, Hirst and Isherwood, *J. Chem. Soc.*, 577 (1937).
[2] Boppel and Freudenberg, *Ber.*, 73, B, 609 (1940).
[3] Hirst and Young, *J. Chem. Soc.*, 1472 (1939).
[4] Malaprade, *Bull. Soc. Chim.*, (5), 1, 833 (1934).
[5] Partridge, *Nature*, 158, 270 (1946).
[6] Flood, Hirst and Jones, *Nature*, 160, 86 (1947).

Structure of Amylopectin

Since starch can be split into maltose, the opinion has been widespread that starch is composed of chains of the maltose type[1]. Some authors, however, suggested a branched or netted structure[2,3]; comparative measurements of molecular weight and viscosity were definitely in favour of a branched structure. Haworth[4], on the other hand, deduced from end-group assays that the molecular weight is low and therefore starch is composed of straight chains of not more than twenty to thirty glucose units. However, such a view could not be maintained. Some years ago it became quite evident[5] that most starches are made up of two polysaccharides: one unbranched, for which we proposed the old name of 'amylose', and one branched, 'amylopectin'.

While the constitution of amylose seems to be well established, the problem of amylopectin constitution is still open. Haworth[4] has proposed a structure in accordance with Fig. 1 which he calls 'laminated', whereas Meyer and Bernfeld[7] favour a structure with irregular branches like the branches of a bush (Fig. 2).

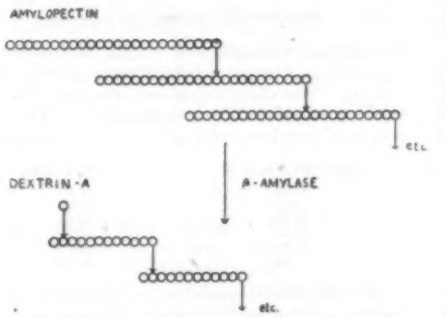

Fig. 1. Structure of amylopectin and limit dextrin according to Haworth

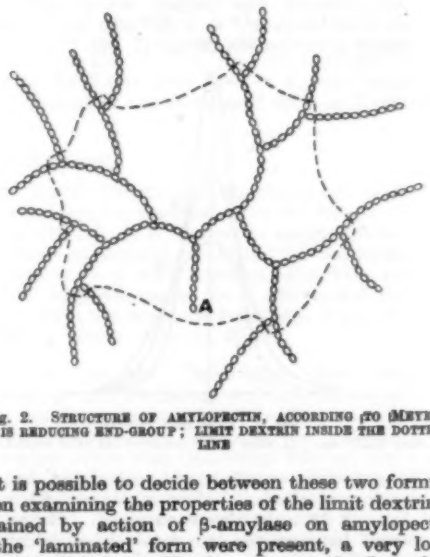

Fig. 2. Structure of amylopectin, according to Meyer. *A* is reducing end-group; limit dextrin inside the dotted line

It is possible to decide between these two formulæ when examining the properties of the limit dextrin *A* obtained by action of β-amylase on amylopectin. If the 'laminated' form were present, a very long-shaped molecule would arise, whereas the 'branched' amylopectin would yield a more compact form

(indicated by the dotted line in Fig. 2). If the acetylated dextrin were composed of fibrillar molecules, its solution would easily be torn into threads, and its films obtained by evaporation would exhibit a considerable solidity such as has been found for amylose acetate and the irregularly constituted lichenin acetate[6]. If it is branched, on the other hand, it would behave like acetylglycogen or analogous products, that is, the films would be extremely brittle. In fact, we found this to be the case: the acetyldextrin evaporated on a glass-plate could not even be detached as film. This behaviour is definitely against the Haworth formula and is in favour of our own assumption.

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July 16.

[1] Meyer, V., and Jacobson, P., "Lehrbuch der org. Chemie", 1, 2 Teil, 1624 ff. (1913).
[2] Meyer, K. H., Mark, H., "Aufbau der Hochpolymeren" (Leipzig, 1930).
[3] Staudinger, H., and Husemann, E., *Ann. Chemie*, **195**, 212 (1937).
[4] Haworth, W. N., *Nature*, **129**, 365 (1932). Hirst, E. L., Plant, M. M. T., and Wilkinson, M. D., *J. Chem. Soc.*, 2375 (1932).
[5] Meyer, K. H., Wertheim, M., and Bernfeld, P., *Helv. Chim. Acta*, **23**, 865 (1940).
[6] Haworth, W. N., *Proc. Roy. Soc.*, A, **186**, 1 (1946).
[7] Meyer, K. H., and Bernfeld, P., *Helv. Chim. Acta*, **23**, 875 (1940).
[8] Meyer, K. H., and Gürtler, P., *Helv. Chim. Acta*, **30**, 751 (1947).

PROF. MEYER's brief sketch of the history of the development of starch structures neglects some essential facts. Before starch could be formulated as consisting of repeating maltose units, proof had to be provided that maltose was preformed in starch and was not a product of synthesis by the enzymes present during starch degradation[1]. Moreover, the constitutional formula of maltose had to be determined before any such formulation of starch could be attempted[2]. The end-group method of assay showed that if starch were represented as a very large molecule, it possessed many end-groups. As early as 1936 [3], in an article on the size of polysaccharide molecules, I made these statements: "it has never been claimed by the present author that the chemical end-group method of determination of chain length corresponds with the particle size", and also "a measurement of particle size must take account of the physical molecule and the molecular aggregate. This may involve not only aggregation to increase the length of chain but also the lateral combination of the chains."

In 1934 [4], I advanced the hypothesis that large molecules containing many end-groups could be formulated by the cross-linking or mutual combination of polymeric units of limited chain-length, and I illustrated this concept in a formula for xylan. In 1937 [5], I advanced the same hypothesis in the laminated formulæ for both starch and glycogen. The nature of this polymeric bond uniting chains of limited length was postulated as an ordinary covalency link although alternatives were not excluded. The nature of this link between adjacent chains was determined later[6]. It has been the procedure of this School to advance formulations only with accompanying experimental evidence. It has been preferred to proceed stage by stage in accordance with the growth of evidence, rather than to present highly speculative formulations with little experimental background.

Evidence that amylose should be represented as continuous chains of ninety or more glucose units, largely unbranched, was published in 1942 [7] after examination of a specimen of the synthetic amylose of C. S. Hanes. This extended and confirmed the view of K. H. Meyer, who had separated amylose by Baldwin's method[8] and had shown that it was completely broken down to maltose by β-amylase, and that it did not give dextrin-*A* as a by-product as does amylopectin[9]. The laminated formula for amylopectin was advanced as a first approximation only, and is not to be regarded as the final or complete structure of amylopectin. It still serves to explain most of the known experimental facts. The multibranched structure advanced by K. H. Meyer is a development of the same concept; but at the present stage of our knowledge it seems to be premature and needlessly complex.

The point made by him above that acetylated dextrin-*A* should, on the basis of our formulation, yield films resembling amylose acetate rests on very slender evidence, and much more adequate experimental investigation will be necessary to support his contention.

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[1] Haworth, W. N., and Percival, E. G. V., *J. Chem. Soc.*, 1342 (1931).
[2] Haworth, W. N., and Peat, S., *J. Chem. Soc.*, 3094 (1926).
[3] Haworth, W. N., *Monatshefte für Chemie*, **69**, 314 (1936).
[4] Haworth, W. N., Hirst, E. L., and Oliver, E., *J. Chem. Soc.*, 1917 (1934).
[5] Haworth, W. N., Hirst, E. L., and Isherwood, F. A., *J. Chem. Soc.*, 577 (1937).
[6] Barker, C. C., Hirst, E. L., and Young, G. T., *Nature*, **147**, 296 (1941), and others.
[7] Haworth, W. N., Heath, R. L., and Peat, S., *J. Chem. Soc.*, 55 (1942).
[8] Baldwin, M. E., *J. Amer. Chem. Soc.*, **52**, 2907 (1930).
[9] Meyer, K. H., and Bernfeld, P., *Helv. Chim. Acta*, **23**, 875 (1940).

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*neo*Pentyl Alcohol and Steric Hindrance

IN a note under this title, Gerrard and Nechvatal[1] have reported the conversion of *neo*pentyl alcohol, $(CH_3)_3C.CH_2.OH$ (here written *R*OH), by thionyl and phosphorus halides into the esters *R*O.SOCl and *R*O.PCl_2, *inter alia*. They remark that Whitmore and Rothrock[2] demonstrated the resistance of *neo*pentyl alcohol to the replacement of its hydroxyl group by halogen, through the agency of thionyl chloride and phosphorus tribromide. They add that we[3] have attributed this to the steric hindrance of what they call "end-on approach" to the α-carbon atom. They state that Whitmore and Rothrock leave the impression that *neo*pentyl alcohol does not react at all with the halides specified; but that the work of Gerrard enabled Gerrard and Nechvatal to predict the probability of an easy "broadside approach" of the reagents. We should like to add some points to this statement of the relation between the different investigations.

Whitmore and Rothrock first showed that *neo*pentyl alcohol exhibits considerable resistance to the substitution of its hydroxyl group *as a whole*, for example, by halogens; though they reported some displacement of the hydroxyl group by means of hydrogen bromide and phosphorus tribromide. They also, however, observed a fairly facile substitution *of the hydrogen atom* of the hydroxyl group in the

formation of an acid ester with sulphuric acid[1]. The difference of behaviour in the two types of replacements has been fully confirmed. We developed more quantitatively the subject of replacements at the α-carbon atom and interpreted the relative difficulty of such reactions in terms of steric hindrance[2]. For example, adopting the conclusion of Cowdray, Hughes, Ingold, Masterman and Scott[4], that the first stage in the formation of halides from alcohols by reagents such as hydrogen bromide, thionyl chloride and phosphorus trichloride is the production of intermediates such as $R\text{OH}_2\text{Br}$, $R\text{OSOCl}$ or $R\text{OPCl}_2$, we adduced evidence[3] that all the routes by which such complexes could normally yield alkyl halides are very much restricted in the *neo*pentyl system. Consequently, such intermediates should be easier to isolate in this system than in others. It is therefore very satisfactory that, while Whitmore and Rothrock had already isolated $R\text{OH}_2\text{Br}$ in this system, Gerrard and Nechvatal have now isolated the esters $R\text{O.SOCl}$ and $R\text{O.PCl}_2$.

The points we wish to make are, therefore: (1) that the reactions which Gerrard and Nechvatal have realized belong to the class (substitution of hydroxylic hydrogen) of those which Whitmore and Rothrock, among others, found to be easy, not the class (substitution of hydroxyl) which they showed to be difficult; (2) that our interpretation[3] of Whitmore's work involved the conclusion which Gerrard and Nechvatal's results support; and (3) that we did not conclude from Whitmore and Rothrock's paper that *neo*pentyl alcohol does not react at all with the reagents specified above. Gerrard and Nechvatal quote as follows from Whitmore and Rothrock's paper: "The inactivity of *neo*pentyl alcohol to heat and reagents has been demonstrated". However, this sentence is taken from the very brief final summary, and was clearly intended as a general and relative statement, not as contradicting part of the text of the paper.

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C. K. Ingold

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Sept. 8.

[1] *Nature*, **159**, 812 (1947).
[2] *J. Amer. Chem. Soc.*, **54**, 3431 (1932).
[3] *J. Chem. Soc.*, 173 (1946).
[4] *J. Chem. Soc.*, 1252 (1937).

Structure of Cyclopropane

Dr. Walsh has defended his suggested structure[1] for the compounds ethylene oxide and cyclopropane in terms of a molecular orbitals picture based on the assumption that the carbon atoms are in the trigonal, or near-trigonal, state. The calculations by Linnett[2] of the CH-bond force-constants are consistent with trigonal bonding.

One would expect to find that the H—C—H angles in cyclopropane and ethylene oxide should lie close to 120° if the carbon atoms are indeed trigonally bonded. It is relevant, therefore, to direct attention to the results of a recent electron-diffraction study on cyclopropane, reported by Bastiansen and Hassel[3]. The photographs were obtained using a rotating-sector camera, and the results are probably more accurate than those obtained by Pauling and Brockway[4] some years ago. Bastiansen and Hassel find the following values for the distance and angle parameters:

$$\text{C—H} = 1{\cdot}08\ \text{A.}; \qquad \text{C—C} = 1{\cdot}54\ \text{A.};$$
$$\angle\,\text{H—C—H} = 118{\cdot}2°; \quad \angle\,\text{H—C—C} = 116{\cdot}4°.$$

The uncertainty in the angles is quoted at $\pm 2°$.

Spectroscopic studies by Smith[5] have provided the value for one of the moments of inertia in cyclopropane ($I_B = 41{\cdot}91 \times 10^{-40}$ gm./cm.^{2}). The calculated moments of inertia of the Bastiansen–Hassel model are $I_A = 68{\cdot}14 \times 10^{-40}$, $I_B = I_C = 42{\cdot}68 \times 10^{-40}$. If the C—C distance in the proposed model is reduced from 1·54 A. to 1·52 A., this alteration in itself is sufficient to give complete agreement with the experimental I_B. Bastiansen and Hassel do not quote the limits of error in the C—C distance determination, which, however, can scarcely be less than $\pm 0{\cdot}02$ A. Some justification for assuming that C—C may be slightly less than 1·54 A. follows from Pauling and Brockway's estimation of C—C = 1·53 ± 0·03 A., and from O'Gorman and Schomaker's[6] estimation of C—C = 1·52 ± 0·02 A. in monochlorocyclopropane.

Although the structural data are not yet complete, the 'most probable' cyclopropane structure would seem to agree with the assumption of near-trigonal bonding, at least in respect of the CH bond-length, force-constant and HCH angle. On the other hand, the C—C distance ($\sim 1{\cdot}54$ A.) corresponds with that of C—C found in the paraffin hydrocarbons, suggesting relatively little double-bond character in the C—C bonds. From the point of view of bond-length, it is satisfactory to represent the carbon–carbon bonding in cyclopropane by single C—C bonds: this representation, however, is not satisfactory in respect of the C—C bond-energy. The thermal data on cyclopropane indicate a marked *weakening* of the C—C bonds relative to C—C in diamond or ethane[7]. In regard to this latter point, it is perhaps significant that the assumption of trigonal bonding in cyclopropane nevertheless leaves a system in which the formation of σ or π C—C bonds occurs under conditions of angular strain.

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Sept. 20.

[1] Walsh, A. D., *Nature*, **159**, 165 and 712 (1947).
[2] Linnett, J. W., *Nature*, **160**, 162 (1947).
[3] Bastiansen and Hassel, *Tids. Kjemi Bergvesen Met.*, **6**, 71 (1946).
[4] Pauling and Brockway, *J. Amer. Chem. Soc.*, **59**, 1223 (1937).
[5] Smith, L. G., *Phys. Rev.*, **59**, 924 (1941).
[6] O'Gorman and Schomaker, *J. Amer. Chem. Soc.*, **68**, 1138 (1946).
[7] See, for example, Skinner, H. A., *Trans. Farad. Soc.*, **41**, 645 (1945).

I am grateful to the Editors for affording me the opportunity of writing a final letter in this discussion. The fundamental idea underlying my earlier letter[1] was that the carbon atoms in cyclopropane and ethylene oxide were in a hybridization state close to that of the carbon atoms in ethylene. The evidence was then strong. Dr. J. W. Linnett has since pointed out that the CH stretching force-constant in these molecules is much closer to the ethylene than to the paraffinic CH_2 value. This developed a point already made, that the height of the strong Raman frequencies indicated the same conclusion. The Bastiansen–Hassel determination of the angle HCH in C_2H_4 as 118° ± 2 provides further strong evidence for my fundamental point. (The smaller angle in the 1,1 dichloro derivative (112° ± 4 [2]) is expected since the angle ClCCl in the comparable 1,1 dichloro ethylene is

reported as several degrees smaller than the angle HCH in ethylene.) Dr. H. A. Skinner's finding of accord between the Bastiansen–Hassel model and the measured moments of inertia reinforces the argument. Evidence from widely separated sources all fits together and shows that ethylene oxide and cyclopropane can be regarded as formed from CH_2 groups the carbon atoms of which are in a state of hybridization close to that of ethylenic carbon atoms. It is this most significant conclusion that a successful theory of these molecules has to explain and the implications of which have to be drawn out.

My suggested representation of the molecules was a natural one to make in order to convey an inkling of the remarkable properties enumerated above and in the second paragraph of my earlier letter[1]. Sir Robert Robinson[2] takes the usual formula to represent only the position in space of the atoms involved. This appears difficult. If we represent a bond C—C, we naturally tend to associate with it properties similar to those of the C—C bond in ethane. If we represent it C=C, we associate it and its neighbours with properties of ethenoid groups and their neighbours. Mr. L. Bateman[4] quotes Ingold as saying symbols are simply intellectual scaffolding. This seems the wiser view. My formulæ form such scaffolding. They help in suggesting the properties of linkages from the ring to other atoms or groups. It is not true that, by making the mental reservation that three resonance structures exist for cyclopropane, 'each unique feature' of the formula 'is destroyed'[2]. The olefinic properties of the CH-bonds, for example, remain. It has to be emphasized, however, that the analogy between cyclopropane and ethylene is primarily one of *valencies*. The *bonds* are only similar if similar valencies are joined to similar atoms. The CH-bonds are similar, but the two C-valencies used to bind *one* other carbon atom in ethylene are used to bind *two* other carbon atoms in cyclopropane. It is not to be expected that the CCC ring of cyclopropane should show, for example, olefinic frequencies. Controversy over the formulæ has tended to arise according as emphasis has been laid on properties of the ring binding or of the bonds external to the ring.

Knowing the approximate valencies used, it follows that the picture of overlapping atomic orbitals in cyclopropane is as (VII) of my earlier letter[1]. On symmetry grounds one then expects the density pattern for the electrons to be as (VIII) of that letter. It is here that Mr. T. M. Sugden's letter[5] is valuable, for it provides a justification of this in molecular orbital theory. His treatment may be simplified by considering only interaction in the ring plane of $2p$ and (separately) sp^2 valencies. Each of these gives rise to three molecular orbitals, one single and two degenerate. But whereas for the sp^2 valencies the single orbital is bonding and the degenerate ones anti-bonding, for the $2p$ valencies the reverse holds. Sugden's diagrams show how this arises. There are thus just three bonding orbitals into which we can fit without difficulty the six CC electrons, so that some light is shed on the stability of cyclopropane. It is easily seen that the occupied molecular orbitals give a density pattern as (VIII) of my earlier letter, two electrons being in a and four being found in b or d or e.

As already stated[1], and as Sugden's Fig. 3 also shows, this pattern can be regarded as the C_2H_4 pattern generalized to an extra dimension. In molecules of this type each carbon atom must have a hybrid atomic orbital pointing towards the molecular centre of gravity and a $2p$ atomic orbital of axis at 90° to this. These essential features are retained if we replace one of the CH_2's by digonal C. The hybrid orbital pointing towards the ring centre is now sp, the two hydrogen atoms are replaced by a second $2p$ orbital of axis at 90° to the ring plane, and a second sp hybrid projects at 180° to the first. One could build a second ring with a plane at 90° to the first. The existence of spiro-pentane with two rings at 90° is thus natural. It is to C_2H_2 as allene is to C_2H_4. Further homologues (in which adjacent rings are always at 90°) are possible by changing other CH_2 groups for digonal C atoms. A special case is the group usually pictured $\begin{matrix} C = C \\ \diagdown\ \diagup \\ \quad C \end{matrix}$, of stability natural on the present, but not on older, views. These matters are described in detail in my forthcoming paper.

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[1] Walsh, *Nature*, **159**, 712 (1947).
[2] O'Gorman and Schomaker, *J. Amer. Chem. Soc.*, **68**, 1138 (1946).
[3] Robinson, *Nature*, **160**, 162 (1947).
[4] Bateman, *Nature*, **160**, 56 (1947).
[5] Sugden, *Nature*, **160**, 367 (1947).

Interaction of Thionyl Chloride and Hydroxy Compounds

Human and Mills[1] have generalized the procedure of Carré and Libermann[2], who prepared, by adding an equimolecular amount of pyridine, the chlorides of the few acids which until then had withstood the action of thionyl chloride. Gerrard and French[3] found it unnecessary to use an equimolecular amount of pyridine in the interaction of thionyl chloride and hydroxy compounds, and proposed to modify the Darzens' procedure[4] by adding the thionyl chloride to a hydroxy compound with a few drops of a tertiary base, and heating for four hours at 65–80° C., which gives them the corresponding chloride.

I pointed out in 1944[5] the catalytic character of the action of pyridine or its hydrochloride: a few drops of it are enough to lower the decomposition point of chlorosulphites some 50–100° C. However, the way the procedure of Gerrard and French is proposed does not demonstrate its universality: these authors chose as examples those alcohols which react with thionyl chloride in a normal way, and do yield alkyl chlorides even without pyridine. It was interesting to see whether that process will permit the preparation of alkyl chlorides with those alcohols which lead to unusual reactions with thionyl chloride. The main interest of Darzens' procedure is its universal application.

The interaction of cyclohexanol and thionyl chloride had given rise to a controversy settled only by Kyrides[6]. Hence we treated cyclohexanol according to Gerrard and French's procedure and only obtained cyclohexene, cyclohexanol and some residual cyclohexyl sulphite. The results were due to the action of hydrogen chloride on cyclohexyl sulphite, in analogy with what we observed[7] in the action of thionyl chloride on phenols; which has been tested by saturating cyclohexyl sulphite with hydrogen chloride. After four hours contact, hydrogen chloride was removed under vacuum and the residue distilled under atmospheric pressure. The whole product had passed over before 160° C., and in that distillate we found only cyclohexene and cyclohexanol.

To see whether that decomposition was not due to pyrolysis, we submitted pure cyclohexyl sulphite to distillation under atmospheric pressure. Decomposition took place, with an abundant evolution of sulphur dioxide; rectification of the distillate showed pure dicyclohexyl oxide, b.p. 270°/760 mm. and 131°/12 mm.

Hence the following modification of Darzens' procedure: 1 mole of thionyl chloride is introduced into 2 moles of hydroxy compound, with circulation of a stream of dry air, until hydrogen chloride ceases to be evolved; 1·2 more moles of thionyl chloride is then added and left for 24 hours. Only then some drops of pyridine are added, and the mixture is heated for four hours on a water-bath to remove the sulphur dioxide. Pyridine only lowers the decomposition point of the corresponding chlorosulphite. Distillation then gives an excellent yield of cyclohexyl chloride (b.p., 141° C.; n_D, 1·462; D^{15}_4, 0·9765).

The same procedure gave us, with yields of nearly 95 per cent, ethyl α-chloropropionate, ethyl phenylchloroacetate, ethyl diphenylchloroacetate.

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[1] Human and Mills, *Nature*, **158**, 876 (1946).
[2] Carré and Libermann, *C.R. Acad. Sci. Paris*, **199**, 1422 (1934).
[3] Gerrard and French, *Nature*, **159**, 263 (1947).
[4] Darzens, *C.R. Acad. Sci. Paris*, **152**, 1314 (1912).
[5] Libermann, *Ion*, **4**, 40–41, 813 (Madrid, 1944).
[6] Kyrides, *J. Amer. Chem. Soc.*, **66**, 1006 (1944).
[7] Libermann, XIth Congress of Pure and Applied Chemistry, London, 1947 (in the press).

Response of New-born Children to Hypertonic Solutions of Sodium Chloride and of Urea

From all that is known about the renal function of new-born infants, it is now clear that their glomerular filtration-rates are considerably lower than those of adults, and also their clearances of urea and sodium chloride[1]. These discoveries have an important bearing on clinical practice, but much more information is required about the way in which the kidney of an infant reacts to changes in the internal environment. Accordingly, some experiments have been made to study the response of the kidney in new-born infants to a considerable rise in the osmotic pressure of the body. The children used for these experiments had all been born with inoperable meningo-myelocœles. Healthy adults have been used as controls.

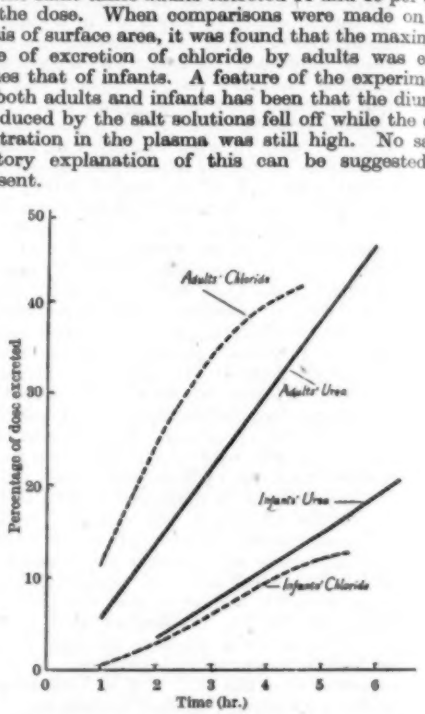
All the subjects were deprived of water for about 12 hours before the tests began. Sufficient sodium chloride was then given intravenously as a 10 per cent solution to raise the serum concentration of chlorine to about 135 m.eq./l. in the adults and to about 147 m.eq./l. in the infants. The results may be summarized as follows:

(1) A diuresis was produced in both infants and adults.

(2) On a basis of surface area, this diuresis was much smaller in infants than in adults. The greatest rate of flow in an infant has so far been 2·7 c.c./1·73 sq. m./min. All adults have exceeded a rate of 7·6 c.c./1·73 sq. m./min., and one has reached a value of 19·1 c.c./1·73 sq. m./min.

(3) The administration of sodium chloride tended to raise rather than lower the osmotic pressure of the urine in infancy, but it greatly reduced the osmotic pressure of the urine in adults[2].

(4) The infants excreted the administered sodium chloride much more slowly than adults (see graph). In one hour they excreted less than 1 per cent of the dose given, and in four hours 9·5 per cent. In the same times adults excreted 11 and 40 per cent of the dose. When comparisons were made on the basis of surface area, it was found that the maximum rate of excretion of chloride by adults was eight times that of infants. A feature of the experiments in both adults and infants has been that the diuresis produced by the salt solutions fell off while the concentration in the plasma was still high. No satisfactory explanation of this can be suggested at present.



Urea has been given by mouth as a concentrated solution to adults and infants in quantities sufficient to raise the blood urea to about 30 m.mol./l. The response at both ages has had the same general characters as the response to sodium chloride (see graph), but the differences between the adults and the infants have not been so great and the maximum output of urea by the adults per unit of surface area has not been more than six times as great as that of the infants. The diuresis, moreover, which at its peak was only about one third as great as that produced by the salt solutions, declined more slowly and the rate of excretion of the urea was better maintained.

It is evident that the kidneys of human infants respond better than those of new-born rats to the administration of strong solutions of sodium chloride and urea[3], but that the responses are essentially similar in nature. It is likely that these signs of immaturity will be demonstrable in all young mammals, but that no two species will show them to the same degree.

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[1] McCance, R. A., and Young, W. F., *J. Physiol.*, **99**, 265 (1941).
[2] Hervey, G. R., McCance, R. A., and Taylor, R. G. O., *Nature*, **157**, 338 (1946).
[3] McCance, R. A., and Wilkinson, E., *J. Physiol.*, **106**, 256 (1947).

Hypertension after Bilateral Nephrectomy in the Rat

Jeffers, Lindauer, Twaddle and Wolferth[1] reported in 1940 that removal of three of the four kidneys in a pair of parabiotic rats resulted in hypertension in the nephrectomized animal. Grollman and Rule[2] found in two pairs of parabiotic rats that removal of the kidneys in one animal sufficed to induce hypertension. When these experiments were repeated by one of us (E. B.-M.) a definite rise in blood pressure was found only in three out of twenty-three pairs, however.

We have modified the procedure by joining three rats in parabiosis, one bilaterally nephrectomized between two normals. In such case the survival-time of the nephrectomized rat is regularly prolonged and it remains in a good general state for a week or longer. In all the six trios prepared, definite hypertension was observed in the nephrectomized animal. A moderate rise in blood pressure was also observed a few times in one or both of the normals. The rise in pressure in the nephrectomized animal occurred two to four days after the removal of the kidneys and the highest figures noted were 156, 144, 170, 190, 172 and 144 mm. mercury.

These results led us to investigate more systematically the blood pressure of single rats after total nephrectomy. A total of 39 normal adult rats were nephrectomized and the blood pressure determined daily until death occurred. A definite rise was observed in 13 rats (33 per cent) (see table); in all these, blood urea was high. In 14 animals a solution of Ringer with 0·5 per cent gelatin was allowed to flow through the peritoneal cavity at a rate of 1 ml. an hour during 1–3 hours, the animal being anæsthetized with nembutal (30 mgm./kgm.) or ether.

BLOOD PRESSURE 1–4 DAYS AFTER NEPHRECTOMY

1	2	3	4
80	120	104	
94	144	90	
	92	130	
90	80	124	
116	80	106*	200
112	116	148	
96	100	136	
104–128	116–116	104	124
100–104	108	96*–128	108
96–100	112	148	
92	80	140*–138	
100– 90	128		
116	128		
110	92*	140	

* Peritoneal washing.

From the table it is evident (1) that the rise in blood pressure mostly occurs during the second or third day after nephrectomy; and (2) that peritoneal washing does not lower the pressure but may increase it. In some cases there were indications of an improvement in the state after washing, though the blood urea showed only a moderate fall.

The results indicate that since hypertension may occur in the absence of kidneys, some extra-renal factor is of importance for the development and maintenance of this type of state of hypertension in the rat. It is tentatively assumed that the normal kidney inactivates some extra-renal factor producing hypertension under certain conditions.

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[1] Jeffers, W. A., Lindauer, M. A., Twaddle, P. H., and Wolferth, C. C., *Amer. J. Med. Sci.*, **199**, 815 (1940).
[2] Grollman, A., and Rule, C., *Amer. J. Physiol.*, **138**, 587 (1942).

Influence of Choline and Ethanolamine on the Rate of Formation of Phospholipids in the Liver

The work described below was carried out in the Biochemistry Department, University of Liverpool, but in consequence of war it had to be abandoned early in 1940. As the results have not been reported elsewhere, it was considered to be of interest to record them.

Using radioactive phosphorus, Chaikoff[1] made an extensive study of phospholipid metabolism, in which he showed that choline caused an increase in the rate of phospholipid turnover in the livers of rats maintained on a 40 per cent fat diet. The maximal increase of 30–40 per cent over the controls was reached six hours after the administration of choline by stomach tube. He suggested that the stimulation of phospholipid turnover was the mechanism by which the choline exerted its lipotropic action. McHenry and Patterson[3] have since supported this suggestion. It occurred to us, however, that this rise in the turnover rate might merely be a mass action effect due to the temporary increase in the concentration of choline, a constituent of the lecithin of the liver phospholipids. If this were the case, it would be expected that the increased rate of turnover would be confined to the lecithin fraction. It would also be expected that the administration, under similar conditions, of ethanolamine, which is not lipotropic[3,4], would also cause a rise in the rate of phospholipid turnover, the increase in this case being confined to the cephalin fraction. As will be seen, this was, in fact, found to occur.

As a preliminary experiment, the relative rates of lecithin and cephalin formation in fed and fasted rats were compared. Five rats on stock diet were injected with radiophosphate in saline, killed six hours later and the organs extracted with ethanol and ether after pooling and grinding with sand. The extracted lipoids were dissolved in ether and fractionated in the usual way. Five rats fasted 22 hours before death were treated similarly. The radioactivity of the samples was measured on a Geiger counter and the specific activity calculated. The specific activity is defined as the percentage of injected radiophosphate which is present per mgm. of phosphorus in the particular phosphatide preparation.

TABLE 1

Tissue	Specific activity of fed rats: Lecithin	Cephalin	Specific activity of fasted rats: Lecithin	Cephalin
Liver	0·42	0·29	0·31	0·21
Kidney	0·31	0·16	0·15	0·10
Small intestine	0·18	0·12	0·13	0·07

It can be seen that though the specific activity of the lecithin is greater than that of the cephalin, the relative increase from the fasted to the fed rats is approximately the same.

The influence of choline on the rates of turnover of the phosphatide fractions in the liver was determined as follows: ten rats were maintained on a 40 per cent fat, 10 per cent albumin diet, and on the seventh day each received an injection of radiophosphate in saline. Five rats received the diet as usual, while the other five had, in addition, 50 mgm.

TABLE 2

Group	Specific activity: Lecithin	Cephalin
Control	0·36	0·24
Choline	0·44	0·26

choline mixed with the diet. Six hours later the animals were killed, and the liver phosphatides extracted and fractionated.

It can be seen that the increase in the specific activity is confined to the lecithin fraction.

In order to compare the effect of ethanolamine feeding with choline feeding, 24 rats were given the high fat diet for six days and injected with radiophosphate on the seventh, as before, but 8 rats received 50 mgm. of choline by stomach tube and 8 rats received 50 mgm. of ethanolamine similarly. The total phosphatides were prepared from the livers and their activity measured.

This experiment was repeated, except that the ethanolamine dose was reduced to equimolar proportions to the choline, namely, 30–50 mgm., and the control group received 1 ml. of distilled water by stomach tube. The phosphatides were fractionated.

TABLE 3

		Specific activity		
	Group	Total phosphatide	Lecithin	Cephalin
1.	Control	0·43		
	Choline	0·67		
	Ethanolamine	0·70		
2.	Control	0·39	0·44	0·35
	Choline	0·52	0·57	0·43
	Ethanolamine	0·69	0·57	0·77

It can be seen that the ethanolamine in both the experiments caused a rise in the specific activity, and therefore in the phospholipid turnover, similar to or greater than that produced by choline, the effect of each base being greatest in the phospholipid fraction of which it was a constituent.

As ethanolamine has no lipotropic action[3,4], it would seem unlikely that choline exerts its lipotropic activity by stimulating phospholipid turnover, unless lecithin plays a much more active part in fat metabolism than cephalin. In view of the similar rise in the specific activity of lecithin and cephalin from fasted to fed rats, this seems improbable. Chaikoff[1] found that methionine, which is lipotropic, caused a rise in the specific activity of liver phospholipids, but he also found that cystine and cysteine, which are negatively lipotropic, produced a similar rise. He suggested that this discrepancy was due to the short duration of the experiment. The same criticisms could be levelled against the results recorded here, but as the effect of choline on the rate of phospholipid turnover is itself lost after 12 hours, and the comparison of lipotropic and non-lipotropic factors was carried out under identical conditions, this seems scarcely justifiable. It would be desirable to extend this work with larger numbers of animals, but the results show that there is no correlation between the lipotropic activity of choline and ethanolamine, and their ability to stimulate phospholipid turnover in the liver.

We wish to thank Dr. J. H. Channon for his advice and encouragement, and Sir James Chadwick, from whom the radiophosphate was obtained, for his advice and assistance with the radioactivity measurements.

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[1] Chaikoff, I. L., *Physiol. Rev.*, **22** (1942).
[2] McHenry, E. W., and Patterson, J. M., *Physiol. Rev.*, **24**, 128 (1944).
[3] Best, C. H., Hershey, J. M., and Hunstman, M. E., *Amer. J. Physiol.*, **101**, 7P (1932).
[4] Platt, A. P., *Biochem. J.*, **33**, 505 (1939).

The Matter-Radiation Cycle in the Cosmos (and the Second 'Law'): the Sun's Equilibrium

IF one scrutinizes the chances of radiation, such as arises even in the observable part of the cosmos, escaping sufficiently to leave as low a temperature for interstellar matter as the 3·2° A. estimated (or even as the temperature of the earth)—on the supposition that nothing but heat (or re-radiation) is produced—one finds that it cannot possibly do so.

Knowing the fraction of a cross-section of the whole that is occupied by nebulæ (some 10^4 light years in virtual radius and 2×10^6 light years apart), and using the very low estimate of 20 per cent absorption in crossing a nebula (it is an average 60 per cent for the unusually large galaxy, at the sun's very eccentric location), it is found that the fraction of radiation penetrating to 5×10^9 light years, the limit of present well-defined observation, is about 0·997, and certainly cannot be as near unity as, say, 0·99999. But 0·997 corresponds to an interstellar temperature of $6{,}000 \times \sqrt[4]{1 - 0{\cdot}997}$, or about 1400° A., and even 0·99999 to 340° A. (Since photographs by the 100-inch reflector show objects that are likely to be nebulæ similarly distributed to a few times 5×10^9 light years, the case is actually stronger than this—observationally; hypothetically, it is stronger again, for one cannot easily think of space, whether infinite or in some way bounded, as unpopulated.)

Therefore, unless we wish to indulge in speculations about some entirely intangible third form of mass, we are forced to conclude that the trapped radiation is converted, mostly in the denser 'shells' of gravitationally and radiatively balanced matter between the stars, into more matter, such as returns to the stars in the manner known of as 'accretion'. Evidently a surface phenomenon, this is easily shown to occur at a rate far too small to be observed in the laboratory, namely, at some 4×10^{12} gm. per sec. in contact with, roughly, 10^{33} gm. After all, why should not a change that occurs at extremely high temperatures be capable of reversal at lower temperatures (one can add, speculatively, possibly at superconduction levels)?

It should be clearer than it is that the second 'law' of thermodynamics, of no account in the microcosmos, is only an appearance in the 'mesocosmos' (where we reside), which is counteracted in the macrocosmos of gravitational 'supremacy', a matter first evident in Jeans' 'gravitational instability'. Dean Inge had no need to write as virtually despairingly as he did in "God and the Astronomers".

A conclusion to be drawn from this is that an average star, such as the sun is providentially ('teleologically' is very like a synonym), tends to keep constant mass, although in the fashion, *cet. par.* (that is, apart from more 'teleology'), of unstable equilibrium; for accretion is proportional to $(\text{velocity})^{-4}$ and velocity is proportional to $(\text{mass})^{-1/3}$.

The subject will be discussed at more length in another place.

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Crystal Structure of Cyanoethyl Cellulose

In the X-ray study of cyanoethyl cellulose filaments[1], made from products of varying cyanoethyl content, it has been found possible to identify at least two forms of crystalline structure. Fig. 1 is an X-ray diagram of filaments made from alkali-soluble material, containing in this case 2·0 per cent nitrogen, corresponding to a substitution of less than one group per glucose residue in the cellulose. The X-ray diagram can be seen to be somewhat similar to that of hydrate cellulose; the normal repeat of pattern of 10·3 A. (approximately) of the cellobiose residue is retained along the fibre axis. It is most probable that the substitution of the cellulose occurs throughout the whole structure, and in these circumstances the obvious falling off in definition of the X-ray diagram is due to distortion of the crystalline lattice, caused by the replacement of certain of the (OH) groups by (OC_2H_4CN) groups. Fig. 2 is an X-ray diagram of acetone-soluble cyanoethyl cellulose filaments containing on an average 2·73 groups per glucose residue; complete cyanoethylation can be represented by the formula: $(C_6H_7O_2.(O.C_2H_4CN)_3)_n$. A preliminary study of this fibre diagram has shown that the crystal structure is built up in the following way.

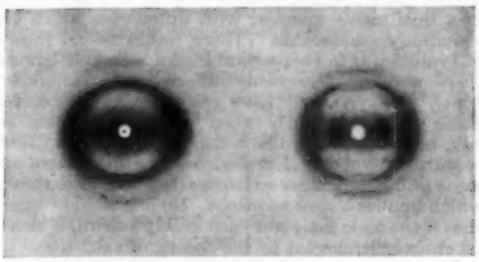

Fig. 1

Fig. 2

Fig. 1. X-RAY PHOTOGRAPH OF FIBRES OF ALKALI-SOLUBLE CYANOETHYL CELLULOSE WITH A NITROGEN CONTENT OF 2 PER CENT. COPPER $K\alpha$ RADIATION. $D \approx 2{\cdot}1$ CM. (ON REPRODUCTION).

Fig. 2. X-RAY PHOTOGRAPH OF FIBRES OF CYANOETHYL CELLULOSE WITH AN AVERAGE SUBSTITUTION OF 2·73 CYANOETHYL GROUPS PER GLUCOSE RESIDUE. COPPER $K\alpha$ RADIATION. $D \approx 2{\cdot}1$ CM. (ON REPRODUCTION).

(1) The period along the fibre axis is $15{\cdot}2_1$ A., which indicates that the repeat is made up of three glucose residues. Hence the cellobiose configuration of the normal structure is not retained.

(2) The structure of the ether presents some difficulties in interpretation, as it is not fully substituted; but the X-ray diagram can be indexed at least to a first approximation on a hexagonal lattice, with the intense reflexions on the basal layer line arising from the (100) plane, and with the (010) and (020) reflexions absent or very weak. A structure may be assigned as follows: $a = c = 9{\cdot}7_4$ A.; $b = 15{\cdot}2_1$ A. (fibre axis); $\beta = 120°$.

This would be consistent with one chain per unit cell, containing three consecutive tricyanoethyl glucose residues arranged on a three-fold spiral along the fibre axis. Further, the above structure has a theoretical density of 1·28 gm./c.c., which is in good agreement with the value of 1·27 gm./c.c. found in white spirit. It is, however, emphasized that this postulated structure is only provisional and may well be a sub-multiple of a larger cell. At present, however, this has been found to be the best possible approximation.

Infra-red studies of the material have shown that in the cyanoethers of low substitution it is possible that some amide groups may be present. It also appears that some of the ethers of equal nitrogen content are soluble in water, whereas others are only soluble in alkali; the cause of this difference is most probably to be found in a more uniform substitution throughout the cellulose structure by cyanoethyl groups in the case of the water-soluble specimens. A final interpretation of the X-ray diagram of tricyanoethyl cellulose must await the elucidation of the problems of the types of ether groups present, together with data from a fully cyanoethylated specimen.

We wish to thank the directors of Messrs. Courtaulds, Ltd., for permission to publish this note.

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[1] Courtaulds, Ltd., and MacGregor, J. H., Brit. Pat. 588,751; and Brit. Pat. Applic. No. 34,508/1945.

X-Ray Diffraction Studies of a Nickel–Thoria–Kieselguhr Catalyst for Fischer–Tropsch Synthesis

A nickel–thoria–kieselguhr (100 : 18 : 100) catalyst prepared mainly according to Fischer and Meyer[1], and rendered completely alkali-free after reduction, by washing with water, was studied by the Debye–Shearer method.

A Debye–Shearer photograph of the kieselguhr used was taken before treatment. This photograph showed a number of sharp diffraction lines and a strong halo. It appears, therefore, that the kieselguhr consists of partly crystalline and partly vitreous or finely colloidal particles. The Bragg angles of reflexion and the spacings corresponding to the diffraction lines are given, along with their eye-estimated intensities, in Table 1.

TABLE 1

Bragg angles of reflexion for copper $K\alpha$ corresponding to diffraction lines	Spacings corresponding to diffraction lines	Eye-estimated intensity
10° 39′	4·20	m.s. (diffuse halo)
13 22	3·37	m.w.
14 48	3·02	m.
15 52	2·81	v.v.w.
17 55	2·54	v.w.
19 42	2·30	w.
21 30	2·11	w.
23 45	1·92	w.
24 21	1·87	w.
28 39	1·62	v.w.
30 20	1·52	v.w.

The X-ray diffraction diagram of a powdered sample of the nickel–thoria–kieselguhr catalyst shows the presence of a few diffraction lines of nickel together with some diffraction lines of the kieselguhr. The weak lines of the kieselguhr having Bragg angles 17° 55′ and 30° 20′ only appeared in this photograph. This shows that in the process of depositing nickel on the kieselguhr the structure of the latter has undergone a change. The two lines of the kieselguhr which appear in this photograph have been identified to be the strong lines of ferric oxide present in the kieselguhr. The lines of silica observed in the photograph

of the kieselguhr are absent in that of the catalyst. It is therefore concluded that during deposition of metallic nickel and thoria on the kieselguhr the crystalline portion of silica (at least, a major part of it) becomes amorphous, while the structure and the crystallinity of ferric oxide present in the kieselguhr remain unchanged. The broadness of the lines, however, indicates that the crystal particles of ferric oxide have been disintegrated, yielding crystallites of smaller size.

At first it was thought that heating of kieselguhr in hydrogen during deposition of nickel and thoria was responsible for the change in its structure. To test this a powder photograph of the sample of kieselguhr previously heated at 450° C. in an atmosphere of hydrogen was taken. This photograph was exactly similar to that of kieselguhr taken before it was heated. This proves that heating alone is not responsible for the change of structure on deposition of the nickel–thoria. A photograph of kieselguhr on which only thoria was deposited according to the Fischer – Meyer method (*loc. cit.*) shows a pattern similar to that on which the nickel–thoria was deposited. The change in the kieselguhr, therefore, was brought about by the disintegration caused by the decomposition of the carbonates of nickel and thorium deposited as an initial step in the preparation of the catalyst.

The powder photograph of the sample of the nickel–thoria–kieselguhr catalyst after it has been used in catalysing higher hydrocarbon synthesis at 180° C. at atmospheric pressure is identical with the X-ray powder diagram of the same sample before use. The presence of nickel lines in the photographs proves that nickel is present in crystalline form in these samples. From the measurement of the glancing angles of reflexion of the nickel lines observed in these photographs, it appears that the lattice constant of nickel has not changed appreciably during its deposition on the kieselguhr. It may, therefore, be concluded that the atomic diameter of the nickel does not change during deposition or during the process of catalysing the Fischer – Tropsch synthesis under our experimental conditions.

The presence of two weak lines of thoria in the thoria–kieselguhr sample proves, on the other hand, the crystallinity of thoria deposited on the kieselguhr.

The X-ray data for the nickel–thoria–kieselguhr catalyst are given in Table 2.

Table 2

Bragg angles of reflexion for copper $K\alpha$ corresponding to diffraction lines	Spacings corresponding to diffraction lines	Eye-estimated intensity	Calculated spacings of possible reflecting planes of Ni	Spacings of ferric oxide	Eye-estimated intensity
17° 44′	2·537	m.w.		2·533	v.st.
22 18	2·029	m.	2·029 (111)		
25 53	1·759	m.w.	1·757 (200)		
30 32	1·520	v.v.w.		1·485	m.s.
38 25	1·239	v.w.	1·243 (220)		
46 40	1·059	v.w.	1·060 (311)		

Our thanks are due to Prof. K. Banerjee for the interest he took in this investigation.

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[1] *Brenn.-Chem.*, **12**, 225 (1931).

The Measurable and the Non-measurable

In the interesting address by Prof. H. Dingle, which appeared in *Nature* of July 26, 1947, there is one section about which considerable difference of opinion may exist. He quotes with disapproval the statement of Eddington ("Nature of the Physical World", p. 275), "The cleavage between the scientific and the extra-scientific domain of experience is not a cleavage between the concrete and the transcendental but between the metrical and the non-metrical", and he names the theory of evolution as completely confounding Eddington's assertion, because the theory of evolution is undeniably a scientific theory but it "has nothing whatever to do with measurement . . . it is concerned with qualitative changes alone . . . yet intelligent and learned men say and believe that science is concerned only with measurement!"

Surely it is not a quibble to deny that science is concerned only with measurement and yet to affirm that the domain of science is the measurable, that is, all the phenomena and experiences for which the fundamental units mass, length, time have relevance. The domain of ethics, æsthetics, the entire realm of values and significance is the non-measurable in the sense that the units $[M]$, $[L]$, $[T]$ are not applicable and have no relevance. The theory of evolution lies within the domain of science because it deals, though in a qualitative manner, with differences which are essentially measurable. There would be no theory of evolution if in the vegetable and animal kingdoms there were no observable differences in factors for which $[M]$, $[L]$, $[T]$ have relevance. An observer notes the difference between a ginkgo tree and an oak, between a crocodile, a horse and a man, without recourse to ruler and scales, but the differences are measurable and their existence has led to a theory which implies a time sequence. No one would suggest that if the time scale were accurately known, it would be entirely irrelevant to the theory of evolution.

The distinction between inquiries in the realm of the measurable and of the non-measurable has helped many people to place science in its right perspective relative to other approaches to knowledge and to truth. Such a generalization may be open to argument, but is it of so dangerous and vicious a nature that "the mask of comedy drops from it and it appears tragical"?

The domain of the non-measurable is a matter of concern for everyone, and very particularly for the man of science who must step outside the domain of his own discipline to play an active part in this matter of ethics, because it is the misuse of science that confronts the present generation with a problem of first magnitude.

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I cannot accept Dr. Douglas's view that because one could, but does not, measure the objects with which biology is concerned, one can therefore claim that the domain of biology is metrical. On those lines one could claim music and painting and poetry as sciences. However, our views do not matter, because Eddington himself has settled the question. He claimed to derive the ultimate laws of Nature from the *nature of measurement*, not from the character of the objects to which measurement was applied, and in his article on the cosmical number, reprinted in his posthumous work "Fundamental Theory", he

proceeded to deduce them from the *details* of the act of measurement—the necessity for four entities such as the two ends of the measuring scale and those of the rod to be measured, etc. He sought to meet the difficulty of biology not by forcing it into the metrical scheme but by denying that it was science and classifying it with theology.

I agree that the distinction between the metrical and the non-metrical has led many people to place science in a perspective; I do not agree that it is the right perspective, and I still think that error on such a matter is tragical, whether it is an error that conforms to our prejudices or not.

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A Rapid Method of Analysis for Certain Surface-Active Agents

WE note with interest a letter by S. R. Epton[1] which describes a partition end-point method for estimation of surface-active agents in solution. We can confirm that such a method gives accurate and reliable results, since a method similar in essentials has been in use in these laboratories for the last two years and was described by one of us in a lecture to students of the Manchester College of Technology in February of this year.

A complete description of our method, which embodies an improvement over the methylene blue method, was submitted for publication to a chemical journal on September 3, 1947.

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[1] *Nature*, **160**, 795 (1947).

"Structure of Spectral Lines"

IN *Nature* of August 23, p. 244, Dr. D. A. Jackson reviews my recent book on "High-Resolution Spectroscopy" (Methuen, 1947). After considerable hesitation and with much reluctance, I feel compelled to comment on several misrepresentations made by Dr. Jackson. That he appears to dislike my method of historical approach is a matter of personal taste with which I do not quarrel, but I cannot allow incorrect assessments of my views to pass unchallenged.

With regard to light sources, his failure to appreciate the wide range and applicability of the various hollow cathode sources I have described is fairly widespread, and indeed this common attitude was a prime reason for their inclusion. By far the biggest volume of published work in hyperfine structure has been made with the different varieties of hollow cathode source.

With regard to the atomic beam as a source, Dr. Jackson quotes me as saying (p. 82), "it is therefore very easy to miss weak components". My complete statement reads, "it is therefore very easy to miss weak components in a structure *if the beam is not dense enough*" (italics added). I think, too, in the circumstances, that the six lines of the review referring to the detection of weak components in potassium (made by Drs. Jackson and Kuhn) could well have been spared, since I give these very details on the following page, with due apportioning of credit.

What astonishes me is the accusation that I understate the superiority of the Fabry–Perot etalon, and I can only conclude that Dr. Jackson has not read my sentence on p. 89: "The Fabry–Perot interferometer has therefore greater power than any of the other instruments in general use". Needless to say, my treatment makes this so obvious that I am quite dumbfounded at Dr. Jackson's failure to appreciate my outlook. I devote 143 pages to instruments: 25 pages to the echelon, 38 pages to the Lummer plate and 80 pages to the Fabry–Perot etalon. If Dr. Jackson does not realize that fact, surely he saw the clear table on p. 88, where I place the Fabry–Perot at the head of the instruments listed in order of practical importance. I feel strongly that misrepresentation in this matter is highly undesirable in the interests of spectroscopy.

Dr. Jackson makes a good deal of play with the reflectivity measurements I have quoted (be it noted, from other workers). I heartily concur when he writes that but few and contradictory measurements are available, but he cannot have it both ways, for he then proceeds to remark: "If the curve had been drawn correctly . . ." May I ask on whose authority, and to which 'correct' curve does he refer? Is it perhaps to some unpublished figures of his own, since he is able to quote figures? Dr. Jackson prefers to select his mirrors with a photo-electric meter (which, incidentally, only measures the absorption, and not the reflectivity, an issue he side-steps), but I still prefer to use 'considered judgment' in my own experiments.

Again, with regard to reflectivity, Dr. Jackson states that sputtering is obsolete; but I have taken pains to show that some quite exceptionally good figures have been reported for sputtering methods, and this should be re-examined. However, I state explicitly on p. 113: "The superiority of the evaporated film over the sputtered film is evident". I personally also only use evaporation.

Finally, Dr. Jackson dislikes mountings of the reflexion echelon other than that he himself uses, and he argues that my discussion of the limitations of his mounting is irrelevant. Surely one might be a little less partisan, and in any case a text-book writer must be.

S. TOLANSKY

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THE criticism which I made about Prof. Tolansky's description of the hollow cathode was that it is "concerned too much with complicated and obsolete forms". I do not fail to appreciate the work which has been done with this light-source in its most important forms, but I criticize the relative amount of space devoted by Prof. Tolansky to obsolete and little-used varieties.

With regard to the atomic beam in absorption, although it is established that this method permits the observation of lines too weak to be detected in emission, Prof. Tolansky does not mention this advantage, but deals at some length with the ease with which weak components are missed if the density

of the beam is low. Obviously, weak components are not observed if the density of the beam is sufficiently low; but it is misleading to write that they are "missed", since if weak components were to be investigated a beam of low density would not be used.

Prof. Tolansky gives a curve for the reflecting power of silver layers, from which he derives values of the reflecting power of the silvered surfaces of an etalon which imply a resolving power too low by a factor of more than two; for this reason I wrote that the "general superiority and greater scope of the [Fabry–Perot] etalon is rather understated". The space devoted by Prof. Tolansky to the Lummer plate is approximately half of the total space given to the etalon and compound etalon. This attaches too much relative importance to the Lummer plate; the results obtained with this instrument compared with those obtained with the etalon are in a very much smaller proportion. The possibilities of the double etalon are understated by Prof. Tolansky. The value of this instrument depends to a great extent on the ratio of the interferometer gaps which can be used, and he states that a ratio of 4 : 1 cannot be exceeded (quoting Meissner, 1941). A ratio of 10 : 1 was used successfully in the examination of the resonance lines of lithium, in which intensity ratios of more than 10 : 1 are involved (Jackson and Kuhn, 1939); but Prof. Tolansky seems to have overlooked this.

With regard to the reflecting power of silvered surfaces, the contradictory measurements available to Prof. Tolansky were those of Strong, which give a reflecting power with red light of about 88 per cent when extrapolated for thick films, and those of Edwards and Petersen (*Phys. Rev.*, **50**, 871; 1936), which gave a value of 98 per cent under similar conditions. Prof. Tolansky has drawn curves (Figs. 7.8 and 7.12) and derived all his conclusions with regard to resolving power of the etalon from the lower of these figures; if he had also considered the higher figure he would have obtained alternative much higher values for the resolving power, and would have seen that Meissner's estimate of the maximum ratio of the gaps in the double etalon, calculated on the basis of a reflecting power of 80 per cent, would be inappropriate. Direct measurements of resolving power have been made at Oxford (and are to be published); the resolving power found for red light with a film with a transmission of 4 per cent requires a reflecting power of 93 per cent, which implies a value of at least 97 per cent for a thick silver layer, in good agreement with the figure given by Edwards and Petersen.

The photo-electric meter measures the transmission of the silver layer for light of any required colour; the resolving powers corresponding to various values of transmission, extending over a large range, have been determined experimentally. The reflecting power can be calculated from the resolving power. Measurement of reflectivity is not therefore an issue which is side-stepped. Furthermore, when the photo-electric method is used, mirrors are not 'selected' after preparation; the density of the silver layer is controlled during the evaporation, and a layer with the requisite degree of transmission is obtained.

Prof. Tolansky appears to agree with my statement that evaporation has replaced sputtering (he "personally also uses only evaporation"); why then does he object to my criticizing him for devoting an approximately equal amount of space in his book to the two methods?

With regard to the mounting of the echelon grating, the aberration which Dr. Tolansky discusses cannot cause a loss of resolving power greater than 10 per cent except with objectives of shorter focal length than is permitted by the grain of the photographic plate; it was for this reason that I described a long discussion of this aberration as irrelevant. I might also have observed that another objection which Prof. Tolansky proposes (that the echelon might be curved and thus have focal properties) is not substantiated by experiment.

D. A. Jackson

Control of Atomic Energy

The article on the "Control of Atomic Energy" in *Nature* of November 15, p. 655, contains an important misstatement of fact that should not be allowed to pass without correction. It is stated: "The rejection by the Soviet Union of the very principles on which these proposals are based, including as they do the prohibition of atomic weapons, makes it difficult to see what further proposals can be advanced with any real hope of unanimous agreement".

This statement could only be taken to suggest that the Soviet Union has taken a stand against the prohibition of atomic weapons. In actual fact, the position is entirely different. Whatever may be the merits or demerits of the other parts of the Soviet plans on atomic energy control, the first point has always been the early conclusion of a convention to outlaw atomic weapons.

The first article of the proposed Convention, put forward by Mr. Gromyko as early as June 19, 1946, states: "The high contracting parties solemnly declare that they will forbid the production and use of a weapon based upon the use of atomic energy, and with this end in view, take upon themselves the following obligations: (*a*) not to use, in any circumstances, an atomic weapon; (*b*) to forbid the production and keeping of a weapon based upon the use of atomic energy; (c) to destroy within a period of three months from the entry into force of this agreement all stocks of atomic energy weapons whether in a finished or semi-finished condition".

The American and British Governments have so far not been willing to agree to such a convention until agreement on the wider aspects of control have been achieved.

E. H. S. Burhop
(Hon. Secretary,
Atomic Sciences Committee).
Association of Scientific Workers,
15 Half Moon Street,
London, W.1.

We regret to find that the article referred to contains an ambiguous statement. What we had in mind was that, according to information generally available, proposals for the control of atomic energy which included the prohibition of atomic weapons had been rejected as a whole by the representative of the U.S.S.R. We had no intention of suggesting that the Soviet Union was opposed to the prohibition of atomic weapons, and we do not think the phrase used need necessarily bear that construction.—Editors.

ORGANISATION OF THE CELL WALLS OF TRACHEIDS AND WOOD FIBRES

By A. B. WARDROP* and Dr. R. D. PRESTON
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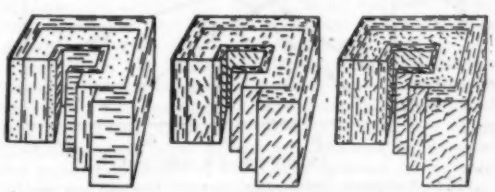
DURING the past thirteen years the finer detail of structure in conifer tracheids has been the subject of some controversy[1,2,3,4]. This has centred round the fact that when transverse sections of conifer wood are examined between crossed Nicols three layers can be distinguished in the secondary wall of a tracheid, an outer and an inner layer which are bright, and a central layer, much thicker in late wood than the other two, which is dark. A similar behaviour is observed in cross-sections of wood fibres and is shown in the case of two isolated fibres of *Nothofagus cunninghami* in Fig. 1. On the basis of observations such as the run of striations in surface view, the orientation of crystals grown in the wall and the features associated with fungal attack, Bailey and his collaborators have claimed that the optical heterogeneity is due to different orientation of the cellulose 'micelles' in the three layers; in that while the micelles in the dark layer are orientated in a steep spiral, those in the outer and inner layers lie more or less transversely[1] or, in later papers, at angles between 45° and 90° to the length of the cell[2,5]. The case where the micelles in the outer layer are orientated transversely and those in the middle layer longitudinally is shown diagrammatically in Fig. 2 (*a*). On the other hand, Preston[3,4,6,7], criticizing the validity of the use of evidence obtained from swollen or otherwise distorted walls and basing his conclusions only on X-ray and optical examination of undistorted material, has maintained that the optical heterogeneity is to be explained largely by a pronounced difference in angular dispersion between the layers (Fig. 2*b*); and that, further, any wall layers with transverse orientation can only be present to an extent so small as to give no record on the X-ray diagrams of conifer wood[7] and to cause no disturbance in the optical properties of walls in surface view[7]. The apparently contradictory X-ray evidence brought forward by Bailey and Berkeley[2] was completely disposed of as an error in interpretation[6], and it seemed at that time that the case was at last clear so far as the physical evidence went.

There still remained, however, some unsatisfactory features. It had been shown by Wardrop[8] that if single walls of tracheids or wood fibres (which were shown by Bailey[1] to manifest similar optical heterogeneity) from abnormal wood with thick cell walls, for example, compression wood in conifers, was crushed and stained with Congo Red, at least two striation directions could be detected, the dichroic properties of which showed that they did, in fact, correspond to two different micellar directions. Since only a slight change in the major extinction position was observed during crushing, no major distortion of structure could have occurred. Although such observations were made on abnormal tissue, they did suggest strongly that the structure of normal tracheids needed re-examination. Further, though little emphasis can be laid on this, it appeared that the birefringence of the outer layer in tracheids was of the same order as that in fibres and this in turn was similar to that of the associated vessels where the micellar orientation is known to be almost transverse[8]. This observation clearly pointed to the danger involved in attempting to determine micellar inclination from the birefringence of a wall in transverse section in terms of the known characteristic birefringence of cellulose. It therefore became very desirable that a sufficiently critical experiment should be designed, to distinguish clearly and irrevocably between the two hypotheses, and it is with the results of what we regard as such an experiment that this communication is concerned.

Fig. 1

Since only one of the hypotheses demands any major net change in orientation between the wall layers, it had long been evident that if the birefringence of a transverse section of a cell could be studied as the section was tilted about a line perpendicular to the direction of observation and to one wall surface, a clear-cut distinction between the hypotheses could be made. Difficulties associated both with technique and the nature of the material made this impossible, but it occurred to one of us (A. B. W.) that the same observations could be made in another way. If a series of sections is cut from a piece of wood at increasing angles to the grain, then a series of observations can be made analogous to those which could have been made on a single, tilting cell. As the tilt of the section increases, the birefringence in one wall will increase, reaching a maximum where the section is parallel to the cellulose chains, and then decrease again. If, therefore, the whole wall is, in fact, uniform in micellar orientation, the birefringence of all layers should show a maximum at the same angle of sectioning; if, on the other hand, it consists of two or more coaxial spirals, then a maximum should be reached at different tilts for each wall layer. Among the disadvantages of this method as compared with the progressive tilting of a single cell it should be pointed out that, first, since different parts of different cells are being examined in different sections, an average birefringence for many cells has to be obtained; secondly, it is imperative that all the tracheids should have spirals of the same sign; and lastly, it is preferable that outer and inner layers should have



(*a*) (*b*) (*c*)

Fig. 2

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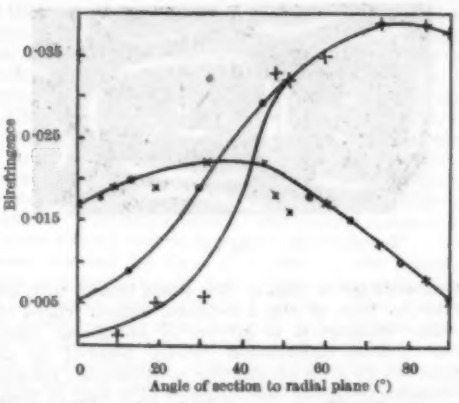

Fig. 3. *Picea*. Outer layer: ×——×, observed; ○——○, calculated. Middle layer: +——+, observed; ●——●, calculated

spirals of the same sign (even if the inclination of the spirals is indeed different). These conditions seemed to be met in a specimen of *Picea* sp. and in the fibres of *Nothofagus cunninghami*, and a series of observations was therefore made on sections from each. In both cases it was found preferable to deal with the tangential walls so that the sections were cut at increasing angles to the radial longitudinal plane. Phase difference determinations were made in sodium light with a de Sénarmont compensator. The section thickness was later determined by turning cells on their sides and measuring directly with an eyepiece micrometer. The birefringence could then be calculated with accuracy. The orientation of the blocks of wood used to the tilting axis was kept constant so that, for example, the tangential wall nearer the pith in each cell of each section was always observed.

The results are shown graphically in Figs. 3 and 4, together with values calculated using the maximum birefringence determined experimentally. The first point to note is that the maxima do not correspond in inner and outer layers. In *Picea* the micelles in the outer layer are, on an average, oriented at an angle about 50° to the long axis of the cells, while in the inner layer the corresponding angle is 18°, comparing favourably with the angle 20° determined from the X-ray diagram. In *Nothofagus* it is somewhat difficult to determine the precise angle in the

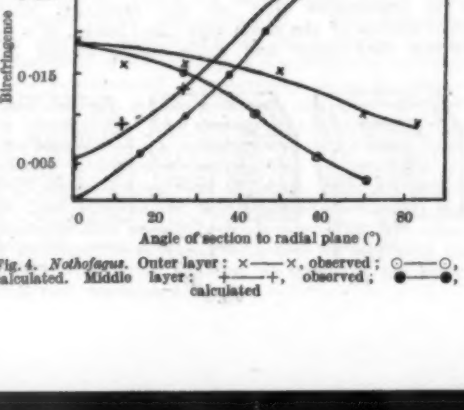

Fig. 4. *Nothofagus*. Outer layer: ×——×, observed; ○——○, calculated. Middle layer: +——+, observed; ●——●, calculated

outer layer—it must lie somewhere between 90° and about 60° to the long axis of the fibres—but it is certainly much greater than the 10° (comparing with the 10° obtained from the X-ray diagram) of the inner layer. It seems, therefore, to be established by the only type of observation which could establish such a result, namely, by observation of the physical properties of untreated and undistorted walls, that the micelles in outer layers of tracheids and fibres, while not truly transverse, do at least form a spiral slower than that of the thicker layer within.

On the other hand, however, it is equally clear that considerable angular dispersion about this mean direction does also occur both in inner and outer layers, and that this also contributes materially to the optical heterogeneity. Thus in the inner layers, the intrinsic birefringence is obviously in the neighbourhood of 0·04, as compared with the 0·06 recorded for cellulose in ramie, cotton, etc. This lower value may be associated partly with the high lignin content of wood walls, but is certainly to be explained largely in terms of dispersion since we have observed a feature first reported by Preston[4], that the major extinction position of the wall as seen in section is often not parallel to the wall surface and is, in fact, sometimes perpendicular to it. Again, the low value of the birefringence (0·02) in the outer layer is probably due in part to the high lignification (much higher than in the middle layer, as shown by staining reactions similar to those used by Coppick and Fowler[9]) but probably reflects considerable angular dispersion. The inner layer is dark in transverse section, therefore, not only because the micellar spiral is a steep one but also because the considerable angular dispersion reduces the birefringence to a level materially lower than it would otherwise have been. Similarly, the bright outer layer is much less bright in transverse section than one would expect, again on account both of high lignification and angular dispersion. Until the determinations reported here were made it was therefore quite unsafe to attempt to make conclusions of any kind as to the interpretation of this particular kind of optical heterogeneity; and it is therefore satisfactory to find that, in fact, both the factors which had previously been stated to be causative are actually involved. The tracheids investigated here are, then, a modification of the type of organisation described by Bailey and Vestal[5], in which both inner and outer layers are spirally orientated, and is represented diagrammatically here in Fig. 2 (*c*).

One final point may perhaps be made. The low birefringence of the outer layers of the tracheids and fibres investigated here, reflecting as it probably does both a low cellulose content and a large angular dispersion, makes it seem rather unlikely to us that true striations should be visible in the outer cell wall layer. It would seem imperative that observation of striation direction should be checked against the optical and other properties of a wall before interpretation is attempted. It is satisfactory to note that the results of the investigation reported here have been used successfully by Wardrop and Dadswell[10] in an investigation of structure in tension wood fibres and compression wood tracheids.

A detailed account of the above investigations will be published later; for assistance with the sectioning we wish to express our indebtedness to the Forest Products Division of the Council for Scientific and Industrial Research, Melbourne, and to Mr. R. Metcalfe, of the Royal Botanic Gardens, Kew. We

are also indebted to Dr. H. E. Dadswell, of the Forest Products Division, C.S.I.R., Melbourne, in whose laboratory the exploratory observations were made, for helpful discussions of the problem.

[1] Kerr, T., and Bailey, I. W., *J. Arnold Arboretum*, **15**, 327 (1934).
[2] Preston, R. D., *Phil. Trans. Roy. Soc.*, B **224**, 131 (1934).
[3] Bailey, I. W., and Berkeley, E. E., *Amer. J. Bot.*, **29**, 231 (1942).
[4] Preston, R. D., *Proc. Leeds Phil. Soc.*, **3**, No. 9, 552 (1939).
[5] Bailey, I. W., and Vestal, M. R., *J. Arnold Arboretum*, **18**, 185 (1937).
[6] Preston, R. D., *Proc. Roy. Soc.*, B **133**, 327 (1943).
[7] Preston, R. D., *Proc. Roy. Soc.*, B **134**, 202 (1947).
[8] Wardrop, A. B., Internal Report Division Forest Products, C.S.I.R., Melbourne, Australia (1946).
[9] Coppick, S., and Fowler, W. T., *Paper Trade J.*, **109**, T.S. 135 (1939).
[10] Wardrop, A. B., and Dadswell, H. E., in the press.

ABSORPTION OF ULTRASONIC WAVES IN LIQUIDS

A WELL-ATTENDED discussion on this topic was held by the Acoustics Group of the Physical Society on November 13 at the Imperial College. The meeting was opened by the chairman, Mr. H. L. Kirke, who said that experimental work in this field had outstripped theory, and that he hoped experimental and theoretical workers would be brought together as a result of the discussion.

Mr. J. M. M. Pinkerton (Cavendish Laboratory, Cambridge) outlined the differences between experiment and the classical theories of Stokes[1] and Kirchhoff[2], first noted by Biquard[3]. The calculations had assumed that the absorption would be caused by viscosity and heat conduction respectively; in liquids the heat conduction term is generally negligible in comparison with the viscosity term. But the experimental behaviour of gases is now accepted as due to a relaxation phenomenon, in the interchange of energy between internal and external degrees of freedom. The absorption α in the majority of liquids has been shown to obey the relation $\alpha/\nu^2 = \text{constant}$, where ν is the frequency of measurement. This is also found for gases when ν is much lower than the relaxation frequency. To pursue the work to higher frequencies and obtain highly accurate data a new method had seemed desirable; from war-time experience the pulse method had appeared to be the most suitable. The first results using this method had been published by Pellam and Galt in 1946 [4]. Mr. Pinkerton's experiments had been made by the same method independently[5]. Results were presented in detail for water. α/ν^2 was found to be constant over the range 7–70 Mc./s., within one or two per cent at constant temperature. There is a fall in α/ν^2 of about nine times between freezing and boiling points. The observed absorption follows the expected temperature variation of Stokes' theory over the range 0°–95° C., although remaining always about three times too large. This is very remarkable.

The behaviour of other liquids, as judged from published data[4–7], can be used to divide them into arbitrary classes: (1) liquids where $\alpha_{obs.}/\alpha_{calc.}$ is 10–1,500, with a relatively small positive temperature coefficient of absorption; (2) those for which $\alpha_{obs.}/\alpha_{calc.}$ lies between 3 and 10, also with a positive temperature coefficient; (3) liquids with $\alpha_{obs.}/\alpha_{calc.}$ between about 1 and 3 and a relatively large negative temperature coefficient; (4) very viscous liquids where the Stokes relation is obeyed. Liquids of classes (1) and (2) are non-associated organic liquids, and class (3) contains the primary alcohols and water, all typically associated liquids. Values of $\alpha_{obs.}/\alpha_{calc.}$ show a nearly regular progression from amyl alcohol to water. Mercury is not anomalous[8], the theoretical absorption being shown up to 1,000 Mc./s. The consequences of a relaxation theory, as calculated by Kneser[9], Mandelstam and Leontovitsch[10] and others, are (*a*) a dispersion of the velocity, (*b*) a maximum in the value of $\mu = \alpha\lambda$; this would correspond to α/ν^2 falling from the constant value observed at lower frequencies. Neither consequence has been verified experimentally. Various ways of estimating the relaxation frequency expected put it in the region of 10^3 Mc./s. for benzene. Hence the lack of experimental proof of a relaxation effect might be accounted for, if the measurements had been made at too low frequencies.

The variation of absorption with concentration in mixtures shows the same distinction between associated and non-associated liquids in the few cases so far investigated. In the case of two pairs of associated liquids, there is an absorption maximum at an intermediate concentration. Four pairs of non-associated liquids show an initial rapid fall, followed by an asymptotic approach to the absorption value of the less absorbing liquid. This can be explained semi-quantitatively by introducing four relaxation times for the mixture and calculating, on certain extreme simplifying assumptions, the mean overall relaxation time as a function of the molecular concentration. The variation in relaxation time has then to account for the change in absorption. In no mixture investigated has a linear variation of absorption been found.

Dr. E. G. Richardson (King's College, Newcastle-on-Tyne) said he had carried out measurements between 100 kc./s. and 1 Mc./s. He outlined the sources of error in many of the experimental techniques used in the past, stressing the undesirable effects of excessive amplitude, and of the diffraction phenomena which occur. The use of very low powers is desirable, and the pulse method fulfils this requirement well. Measurements, using a hot-wire detector, of the diffraction pattern of a source at low frequencies have confirmed the absence of scattering, mentioned by Mr. Pinkerton. Measurements of the Doppler frequency shift of light scattered by various liquids had led Raman and Rao[11] to the conclusion that the velocity of 'hypersonic' waves (identifiable with Debye heat waves) is less than that measured at low frequencies. He was uncertain if it could be shown that all the ultrasonic energy absorbed appears as heat. A method of measuring absorption at low frequencies by propagating waves in a tube might be developed. Prof. L. Tisza had suggested that the observed behaviour of liquids could be represented by the introduction of a second coefficient of viscosity[12]. This point was taken up by Dr. Temperley later in the discussion. Another theory, in which the compressibility is relaxational, has been developed for water by L. Hall[13]. Dr. Richardson was concerned that any theory to explain the absorption behaviour of liquids should not introduce too many arbitrary constants, incapable of being verified independently.

Dr. H. N. V. Temperley (Cavendish Laboratory) discussed the possibility of a coefficient of expansion viscosity. Mathematically this is easily defined, and was introduced in his calculations by Stokes. This coefficient had been shown by Maxwell to be zero for gases, but seems to have been ignored for liquids. There are several theoretical models which might be used to calculate the absorption, based on the idea

of a potential cell. Such models can in all cases lead to a finite expansion viscosity. A relaxation time can be involved in the readjustment of the molecules in potential cells to a small change in external conditions. He thought that any explanation of the behaviour of water must explain the variation of both the viscosity and the absorption with temperature, since these are so closely related. The distinctions drawn between associated and non-associated liquids make it clear that different theories must be developed for the two cases.

In the discussion contributions were made by Dr. P. Vigoureux, Mr. Bayard, Mr. D. O. Sproule, Dr. J. Lamb, Mr. Ribchester and Mr. E. Bauer. Dr. Lamb referred to the theoretical papers by Dutta[14], and Dutta and Ghosh[15] on dispersion and absorption of ultrasonic waves in liquids, and measurements by Rapuano[16] up to 250 Mc./s. Venkatswaran[17] had not confirmed Raman and Rao's results for dispersion of hypersonic waves.

[1] Stokes, G. G., *Trans. Camb. Phil. Soc.*, **8**, 287 (1845).
[2] Kirchhoff, G., *Pogg. Ann.*, **134**, 177 (1868).
[3] Biquard, P., *Ann. de Physique*, **6**, 195 (1936).
[4] Pellam, J. R., and Galt, J. K., *J. Chem. Phys.*, **14**, 608 (1946).
[5] Pinkerton, J. M. M., *Nature*, **160**, 128 (1947).
[6] Willard, G. W., *J. Acous. Soc.*, **12**, 438 (1941).
[7] Hunter, J. L., *J. Acous. Soc.*, **13**, 36 (1941).
[8] Ringo, G. R., Fitzgerald, J. W., and Hurdle, B. G., *Phys. Rev.*, **72**, 87 (1947).
[9] Kneser, H. O., *Ann. d. Phys.*, **32**, 277 (1938).
[10] Mandelstam, L., and Leontovitsch, M., *J. Exp. Theor. Phys.*, **7**, 438 (1937).
[11] Raman, C. V., and Rao, B. V. R., *Nature*, **135**, 161 (1935).
[12] Tisza, L., *Phys. Rev.*, **61**, 531 (1942).
[13] Hall, L., *Phys. Rev.*, **71**, 318 (1947).
[14] Dutta, A. K., *Trans. Bose Research Inst.*, **12**, 115 (1936–37).
[15] Dutta, A. K., and Ghosh, *Trans. Bose Research Inst.*, **13**, 30 (1937–38).
[16] Rapuano, R. A., *Phys. Rev.*, **72**, 78 (1947).
[17] Venkatswaran, C. S., *Proc. Ind. Acad. Sci.*, **15**, 371 (1942).

FUTURE OF AGRICULTURE IN BRITAIN

IN a Friday evening discourse at the Royal Institution on October 31, Lord De La Warr discussed the prospects of British agriculture and how far it could help to meet the country's present problems of a shortage of dollars and the threat of an actual scarcity of food. The first essential, he said, is to decide the true nature of the trouble, whether it is a 'crisis' of temporary duration only or whether we are not rather being confronted with a fundamental change in our national economy. If the difficulties are merely short-term, it would seem that British agriculture has little contribution to make, since production has declined over the last two years both as regards acreage of tillage and livestock figures, and time will be needed to reverse this trend. Though increase of livestock may be the best way of farming to save dollars, the opportunity, which offered itself twelve months ago, of buying adequate feeding stuffs to do this had not been taken, so that the prospect of building up an animal population rapidly is not hopeful. During the last two years the whole world food situation has gone from bad to worse, and though this may be no one's fault, it must be admitted that interest in agricultural matters has been sadly lacking in the British nation as a whole. The word 'crisis' is, in fact, quite misleading, and its use has prevented us from realizing that the start we gained in the industrial revolution has run out and that cheap food and raw materials, with the consequent high standard of living to which we had become accustomed, can no longer be taken for granted. The idea that when the present 'crisis' ends we shall again be able to import all the food we require at a low price must be abandoned before we can hope to rebuild our shattered economy.

During the last five years before the War, Great Britain was importing on an average £50 million per annum more than it was exporting; that is, we had already begun to live beyond our means, and since then we have raised our standard of living. Countries such as Canada, Australia and South America, that in the past were mainly food producers, have been steadily industrializing themselves, thereby decreasing their demands for British exports. Great Britain is, in consequence, not likely to be in a financial or economic position to pay for importing as large a proportion of food as formerly, and may even have difficulty in exporting sufficient to pay for essential raw material to maintain her factories. It is questionable, moreover, whether the food is even available, assuming we had the money to pay for it. Besides the changes brought about by the industrialization of the great food-producing countries, there are those due to alterations in farming methods. The switch-over from cereal growing to livestock production that has occurred in the 'dust bowl' areas of the United States and Canada means less grain available for human requirements. Furthermore, enormous increases in population are taking place in India and Africa, as well as a rise in the consumption per head in these countries, due to the efforts to improve the standards of health and nutrition among the native peoples.

Other factors may prove of importance in determining future trends. The control of the tsetse fly and locust, for example, would open up new regions for agricultural development, which in their turn might temporarily offset this increased demand for food. On the other hand, the elimination of the mosquito would result in an even more rapid rise in population in many areas.

For the purpose of feeding the people of Great Britain, the products of British agriculture are, therefore, literally of vital importance. The increase of £100 millions of food by 1952 demanded by the Government is moderate from the point of view of the potentialities of the soil and the needs of the situation. In terms of the facilities now available, of labour, houses, machinery, fertilizers and feeding stuffs, however, the target is not likely to be reached unless the drive behind it is intensified. It is the country, not the farming industry, that will suffer if the necessary increased production is not attained, and the facts and figures available point to the strong possibility of real hunger being experienced during the next twelve months. However, the soil of Britain is capable of producing more. Four years ago, 1½ million acres more were under cultivation than to-day, and there were more pigs and sheep and about the same number of cattle.

More knowledge and better application of existing knowledge are needed. Means must be found for carrying over new information particularly to the small farmer, and an improvement in the national standard of education is probably the best solution to this problem. Compared with other countries, the farming efficiency in Britain is high; more is produced per head than in any other country in Europe,

and we grow 2½ times as much wheat per acre as in the United States or Canada. We are highly mechanized and in many cases our costs compare well with what we have to import. Both soil and climate are in our favour and our workers are good. The root of our trouble is the national unwillingness to realize that this is no 'crisis' with which we are faced, but a long-term, if not permanent, change. Not until this is completely accepted will the requisite national energy be put behind our agricultural drive. Labour, houses, machinery, fertilizers and feeding stuffs are our immediate needs. But they are only the beginning. Whatever our shortages and difficulties may be, the countryside must be re-equipped with new buildings, houses, electricity, water and transport. We need the best that science can give, and we are gradually drawing into agricultural research some of the best brains that science can produce. But we need more; in fact, if the land of Great Britain is to yield of its best, it will demand a greater share of every section of our national wealth, its energies, capital, income, raw material and, above all, its brains.

Even so, it will take time to achieve the necessary increased production, and when we have achieved it, we shall not have solved the whole problem of feeding the 45 million people who inhabit Great Britain. Neither agriculture nor any other single industry can provide a panacea. In fact, one of the greater lessons that this generation will have to learn from the coming hard times is to give up this passionate belief that panaceas exist. But granted the readiness of the nation to change, and change fundamentally, its whole attitude to agriculture, there is no other industry in the country that can make as great a contribution—long-term contribution—to the future of Great Britain.

PATHOLOGY OF GLASSHOUSE CROPS

THE thirty-first annual report for 1945 of the Cheshunt Experimental and Research Station (Turner's Hill, Cheshunt, Herts, 1946) sets forth the results of several pieces of research into the pathology of glasshouse crops.

P. H. Williams has investigated the effect of temperature and humidity on the pathogenicity of *Verticillium albo-atrum* and *V. dahliæ*. He finds that *V. albo-atrum* made very little progress when inoculated to tomato plants growing in a closed chamber, with temperature and humidity higher than normal. The plants did not wilt in this environment. *V. dahliæ*, on the other hand, caused about the same amount of disease in a closed chamber and under normal conditions. *Verticillium* wilts do not appear to owe greater severity of attack to increasing soil moisture. Cucumber plants wilted under conditions which checked the disease due to *V. albo-atrum* in tomatoes.

The fungus *Didymella lycopersici* causes a severe stem rot of tomato plants. Mrs. Enid Sheard has shown that the disease can spread by air-borne spores, that an attack is more severe where plants are grown in soil which has been steam-sterilized before inoculation than in unsterilized soil, and that the fungal strain which attacks only the fruit of outdoor tomatoes will not cause stem rot under glass. With O. B. Orchard, Mrs. Sheard has found that aerial infection is controlled by fumigation with formaldehyde vapour. W. H. Read has also attempted control of the disease by applying various fungicides to the surface soil of glasshouses, before and during planting.

I. W. Selman has found that fruiting tomato plants may show marked resistance to invasion by mosaic virus, and that inorganic nutrition also has an effect in promoting resistance to mosaic. Experiments are in hand to determine the nutrient balance most favourable to resistance. An interesting application for disease control by antibiotics is investigated by Erma Grossbard. A strain of *Aspergillus clavatus* has been used to depress the incidence of 'damping off' of tomato seedlings by the fungus *Phytophthora cryptogea*. Antibiotic properties were also demonstrated against *Bacillus carotovorus*. Further trials with petroleum emulsion for the control of red spider mite have been made by E. R. Speyer and W. J. Parr. Large-scale use of D.D.T. has proved of value against the tomato moth, but applications of sodium ethyl xanthate against root-knot eelworm have not provided much hope of commercial control.

Several non-pathological investigations have also been made. They include the Station's continued work on general cultivation, an evaluation of the use of sulphur and sulphuric acid for making alkaline soils more acid, the choice of extractants in soil analysis, and the effect of plant hormones on the setting of tomato fruit.

FORTHCOMING EVENTS

Wednesday, December 31

ROYAL MICROSCOPICAL SOCIETY, SECTION OF INDUSTRIAL MICROSCOPY (in the Hastings Hall, B.M.A. House, Tavistock Square, London, W.C.1), at 5.30 p.m.—Mr. F. D. Armitage: "Microscopy of Paper".

APPOINTMENTS VACANT

APPLICATIONS are invited for the following appointments on or before the dates mentioned:

ENGINEER IN THE AUDIO FREQUENCY SECTION of the Research Department, based in London, ENGINEERS (2) IN THE FIELD STRENGTH SECTION of the Research Department, based at Oxford, ENGINEERS (senior and junior) IN THE DESIGNS DEPARTMENT in London—The Engineering Establishment Officer, B.B.C., Broadcasting House, London, W.1 (January 3).

ASSISTANT CHEMISTS (2) IN THE CHEMISTRY BRANCH of the Department of Agriculture and Lands, Southern Rhodesia—The Office of the High Commissioner for Southern Rhodesia, 429 Strand, London, W.C.2 (January 5).

SUPERINTENDENT IN THE SHIP DIVISION of the National Physical Laboratory in the Department of Scientific and Industrial Research—The Secretary, Civil Service Commission, Scientific Branch, 27 Grosvenor Square, London, W.1, quoting No. 2065 (January 7).

RANKIN LECTURER (Grade I) IN THE PREHISTORIC ARCHÆOLOGY of WESTERN EUROPE, and a CHARLES W. JONES LECTURER (Grade II) IN THE DEPARTMENT OF ANCIENT HISTORY AND CLASSICAL ARCHÆOLOGY—The Registrar, The University, Liverpool (January 8).

LECTURER IN ELECTRONICS in the University of Adelaide—The Secretary, Universities Bureau of the British Empire, 8 Park Street, London, W.1 (January 12).

PRINCIPAL SCIENTIFIC OFFICER at the Royal Air Force Institute of Aviation Medicine, Farnborough, Hants, under the Air Ministry—The Secretary, Civil Service Commission, Scientific Branch, 27 Grosvenor Square, London, W.1, quoting No. 2071 (January 12).

CHIEF CONSERVATION OFFICER in charge of the Conservation Branch of the Irrigation Department, Southern Rhodesia—Office of the High Commissioner for Southern Rhodesia, 429 Strand, London, W.C.2 (January 21).

DIRECTOR OF MUSEUMS—The Town Clerk, Municipal Buildings, Dale Street, Liverpool 2, endorsed 'Director of Museums' (January 26).

CHEMIST for an investigation of the isolation and analysis of the insecticidal constituents of pyrethrum flowers—Dr. S. H. Harper, Chemistry Department, King's College, Strand, London, W.C.2 (January 31).

HEAD OF THE DEPARTMENT OF PHYSICS (Senior Lecturer)—The Secretary, St. Mary's Hospital Medical School, Paddington, London, W.2 (January 31).

DIRECTOR—The Director, National Museum of Wales, Cardiff (February 28).

LECTURER IN THE DEPARTMENT OF ELECTRICAL ENGINEERING AND PHYSICS—The Principal, Borough Polytechnic, Borough Road, London, S.E.1.

TECHNICIAN in the City Hospital Laboratory, Urquhart Road, Aberdeen—Dr. Smith, The Laboratory, City Hospital, Aberdeen.

DIRECTOR OF RESEARCH—The British Hydromechanics Research Association, 94–98 Petty France, London, S.W.1.

LABORATORY TECHNICIAN (male)—The Secretary, Department of Parasitology, School of Tropical Medicine, Pembroke Place, Liverpool 3.

HEAD OF THE SCIENTIFIC DIVISION—The Director of Research, British Boot, Shoe and Allied Trades Research Association, Satra House, Rockingham Road, Kettering, Northants.

REPORTS and other PUBLICATIONS

(not included in the monthly Books Supplement)

Great Britain and Ireland

University of London. Report by the Principal on the Work of the University during the Year 1946–47. Pp. 10. (London: University of London, 1947.) [75

Ministry of Agriculture and Fisheries. Register of Accredited Poultry Breeding Stations and Accredited Hatcheries, 1947. Pp. 194. (London: Ministry of Agriculture and Fisheries, 1947.) [75

Air Ministry. Air Publication 3139: Psychological Disorders in Flying Personnel of the Royal Air Force investigated during the War, 1939–1945. Pp. iv + 344. (London: H.M. Stationery Office, 1947.) 7s. 6d. net. [75

Flax Diseases: an Illustrated Bulletin on their Recognition in the Field. By Prof. R. McKay. Pp. x + 56 + 22 plates. (Dublin: Flax Development Board, 1947.) 5s. [75

Philosophical Transactions of the Royal Society of London. Series A, Mathematical Sciences, No. 917, Vol. 240: Divergent-Beam X-Ray Photography of Crystals. By Dr. Kathleen Lonsdale. Pp. 219–250. 7s. Series B, Biological Sciences, No. 590, Vol. 232: The Innervation and Actions of the Neurohypophysis, an Investigation using the Method of Remote-Control Stimulation. By G. W. Harris. Pp. 385–441. 11s. 6d. (London: Cambridge University Press, 1947.) [75

General Medical Council. Recommendations as to the Medical Curriculum, with an Introduction. Pp. 56 + 12. (London: Constable and Co., Ltd., 1947.) 2s. 6d. [85

University of London: University College. Annual Report, February 1946–February 1947. Pp. ii + 208. (London: Taylor and Francis, Ltd., 1947.) [85

London School of Hygiene and Tropical Medicine. History of the Library. Pp. 24. (London: London School of Hygiene and Tropical Medicine, 1947.) [85

A British Refinery in Wartime. Pp. 16. (London and Manchester: Manchester Oil Refinery, Ltd., 1947.) [85

Textile Institute. Annual Report, Balance Sheet and Accounts for 1946. Pp. 12. (Manchester: Textile Institute, 1947.) [85

The Aircraft Builders: an Account of British Aircraft Production, 1935–1945. Pp. 96. (London: H.M. Stationery Office, 1947.) 9d. net. [85

Association of Special Libraries and Information Bureaux. Report of ASLIB Meeting on Planning and Equipment of Special Libraries, 6 April 1946. Pp. 33. (London: Association of Special Libraries and Information Bureaux, 1947.) 3s.; to non-Members, 4s. [85

Positex Pamphlet, No. 2: The Compounding, Pigmenting and Thickening of 'Positex'. By Dr. C. M. Blow. Pp. 8. (London: British Rubber Development Board, 1947.) [85

Report of the Local Government Boundary Commission for the Year 1946. Pp. 20. (London: H.M. Stationery Office, 1947.) 4d. net. [85

South-West Essex Technical College and School of Art. Annual Report, Session 1945–46. Pp. 31. (London: South-West Essex Technical College and School of Art, Walthamstow, 1947.) [85

Gas Research Board. 8th Annual Report of the Council of the Gas Research Board, 1946. (Communication GRB. 30). Pp. 62. (London: Gas Research Board, 1947.) [85

London School of Hygiene and Tropical Medicine. Report of a Meeting of the Rose Institute Industrial Advisory Committee. Pp. 36. (London: London School of Hygiene and Tropical Medicine, 1947.) [85

Anti-Locust Memoir 2: Migrations and Breeding of the Red Locust (*Nomadacris septemfasciata* Serville) in Africa, 1927–1945. By V. Morant. Pp. 60. (London: Anti-Locust Research Centre, British Museum (Natural History), 1947.) 6s. [85

British Trust for Ornithology. Thirteenth Report for the Year 1946. Pp. 24. (Oxford: British Trust for Ornithology, 1947.) [85

Forestry Commission. Booklet No. 1: Woodland Mosses. By H. Watson. Pp. 48. 2s. net. Forest Operations Series, No. 2: The Establishment of Hardwoods by Sowing or Planting. (Pp. 36. 9d. net. Leaflet No. 19: Elm Disease, *Ceratostomella ulmi*. Revised edition. Pp. 8. 2d. net. (London: H.M. Stationery Office, 1947.) [85

Laboratory Assistants in Schools. Report of a Sub-Committee of the Essex Science Teachers Association. Pp. 8. (Chelmsford: Essex Science Teachers Association, Mid-Essex Technical College, 1947.) 6d. [85

Other Countries

Annals of the New York Academy of Sciences. Vol. 48, Art. 6: The Relation of Diseases in the Lower Animals to Human Welfare. By William A. Hagan, Herald R. Cox, William H. Feldman, L. Forrest Huddleson, Harald N. Johnson, Raymond A. Keiser, Joseph V. Klauder, Karl F. Meyer, C. D. Stein and Willard H. Wright. Pp. 351–576 + 11 plates. (New York: New York Academy of Sciences, 1947.) [75

Bulletin of the American Museum of Natural History. Vol. 88, Article 4: Merychyinae, a Subfamily of Oreodonts. By C. Bertrand Schultz and Charles H. Falkenbach. Pp. 157–286. Vol. 88, Article 6: An Analysis of the Geometry of Symmetry with especial reference to the Squamation of Fishes. By C. M. Breder, Jr. Pp. 321–412 + plates 15–16. Vol. 89, Article 1: Cretaceous and Tertiary Actinopterygian Fishes from Brazil. By Bobb Schaeffer. Pp. 40 + plates 1–7. (New York: American Museum of Natural History, 1947.) [75

Department of State. Publication 2615 (Conference Series 88): Report of the West Indian Conference, Second Session, St. Thomas, Virgin Islands, U.S.A., February 21 to March 13, 1946. Pp. v + 88. (Washington, D.C.: Government Printing Office, 1947.) [75

Publikace Pražské Státní Hvězdárny (Publications de l'Observatoire National de Prague). C. 18: Extension des tables dioptriques de l'atmosphère terrestre; Exploration météorologique de la haute atmosphère. Par F. Link. Pp. 16 + 12. (Prague: Státní Hvězdárna, 1947.) [75

Svenska Linné-Sällskapets Årsskrift. Årgång 29, 1946. Pp. iv + 134. (Uppsala: Almquist and Wiksells Boktryckeri A.-B., 1946.) [75

Smithsonian Institution: United States National Museum. Contributions from the United States National Herbarium, Vol. 28, Part 1: A Botanical Bibliography of the Islands of the Pacific, by Elmer D. Merrill; A Subject Index to Elmer D. Merrill's 'A Botanical Bibliography of the Islands of the Pacific', by E. H. Walker. Pp. v + 404. (Washington, D.C.: Government Printing Office, 1947.) 1 dollar. [75

Transactions of the American Philosophical Society. New Series, Vol. 37, Part 1: Postglacial Forest Succession, Climate and Chronology in the Pacific Northwest. By Assoc. Prof. Henry P. Hansen. Pp. ii, + 130. (Philadelphia: American Philosophical Society, 1947.) [75

University of Bombay: Department of Chemical Technology. Annual Report, 1945–46. Pp. vi + 31. (Bombay: The University, 1947.) [75

Smithsonian Miscellaneous Collections. Vol. 106, No. 18: On the Evolutionary Significance of the Pycnogonida. By Joel W. Hedgpeth. (Publication 3866.) Pp. ii + 53 + 1 plate. Vol. 106, No. 20: A Monograph of the West Atlantic Mollusks of the Family Acilidae. By Paul Bartsch. (Publication 3868.) Pp. ii + 29 + 6 plates. Vol. 107, No. 4: The Sun's Short Regular Variation and its Large Effect on Terrestrial Temperatures. By C. G. Abbot. (Publication 3892.) Pp. ii + 33. (Washington, D.C.: Smithsonian Institution, 1947.) [75

Proceedings of the United States National Museum. Vol. 96, No. 3205: The Phorid Flies of Guam. By G. E. Bohart. Pp. 397–416. (Washington, D.C.: Government Printing Office, 1947.) [75

Chicago Natural History Museum. Zoological Series, Vol. 31, Part 3: A Bibliography of Birds, Part 3, Subject Index. By Prof. Reuben Myron Strong. (Publication 581.) Pp. 528. (Chicago: Chicago Natural History Museum, 1946.) 4.50 dollars. [75

United States Department of Agriculture. Leaflet No. 161: The Eastern Tent Caterpillar. By F. M. Wadley. Pp. 6. (Washington, D.C.: Government Printing Office, 1947.) 5 cents. [75

University of Colorado Studies. Series C, Studies in the Social Sciences, Vol. 1, No. 4: Editorial Opinion in the Contemporary British Commonwealth and Empire. By James G. Allen. Pp. xxii + 297–606. (Boulder, Colo.: University of Colorado, 1946.) 1 dollar. [75

City of Durban: Durban Museum and Art Gallery. Annual Report for the Year ended 31st July, 1946. Pp. 8 + 4 plates. (Durban: Durban Museum and Art Gallery, 1947.) [75

Herbertia. Vol. 12 (1945). Edited by Hamilton P. Traub. Pp. 198. (Stanford University P.O., Calif.: American Plant Life Society, 1947.) [75

Index to the Literature on Spectrochemical Analysis. Part 2, 1940–1945. By Bourdon F. Scribner and William F. Meggers. Pp. iii + 180. (Philadelphia: American Society for Testing Materials, 1947.) 3 dollars. [75

Chicago Natural History Museum. Popular Series, Botany, No. 28: Tropical and Subtropical Fruits. By B. E. Dahlgren. Pp. x + 72. (Chicago: Chicago Natural History Museum, 1947.) 50 cents. [75

Bulletin of the Museum of Comparative Zoology at Harvard College, Vol. 99, No. 1: Review of the Labyrinthodontia. By Alfred Sherwood Romer. Pp. 368. (Cambridge, Mass.: Harvard College, 1947.) [75

Indian Central Cotton Committee; Technological Laboratory, Technological Bulletin, Series A, No. 65: Technological Reports on Trade Varieties of Indian Cottons, 1946. By D. L. Sen. Pp. viii + 100. (Bombay: Indian Central Cotton Committee, 1946.) 1·8 rupees. [75

Rockefeller Foundation. A Review for 1946. By Raymond B. Fosdick. Pp. 64. (New York: Rockefeller Foundation, 1947.) [75

Bulletin of the American Museum of Natural History. Vol. 88, Article 5: A Contribution to the Visceral Anatomy, Development and Relationships of the Plectognathi. By C. M. Breder, Jr., and Eugenie Clark. Pp. 287–320 + plates 11–14. (New York: American Museum of Natural History, 1947.) [75

Illinois State Geological Survey. Circular No. 126: Operation of the Illinois State Geological Survey. By M. M. Leighton. Pp. 8. (Urbana, Ill.: Illinois State Geological Survey, 1946.) [75

Catalogues

Scientific Works, including Books in Botany, Geology, Zoology, Natural History, Ornithology, Mathematics, Physics, etc., and Periodicals. (Catalogue No. 503.) Pp. 34. (Cambridge: Bowes and Bowes, 1947.)

International Medical Journals. Pp. 16. (Basel: S. Karger, 1947.)

Pocket Stereoscope for Planning, Examination, Re-Development, Map Revision, Surveys. (Leaflet 682.) Pp. 2. (London: Casella and Co., Ltd., 1947.)

Natural History. (List No. 202.) Pp. 28. (Brighton: John Tiranti, Ltd., 1947.) 2d.

A Catalogue of Books on a Large Variety of Subjects. (Catalogue No. 683.) Pp. 142. (London: Francis Edwards, Ltd., 1947.)

Books mainly on Economics and Economic History from the Library of the late Sir John Clapham. (Catalogue No. 502.) Pp. 70. (Cambridge: Bowes and Bowes, 1947.)

'Cintel' Six-Channel Photographic Recorder. Pp. 4. Preliminary Announcement of the 'Cintel' Electronic Metal Detector (Type T2A). Pp. 2. 'Cintel' Electronic Timer, Type 3. Pp. 2. 'Cintel' Process Timer. Pp. 4. 'Cintel' Six-Channel Electronic Recorder. Pp. 4. 'Cintel' Radiation Recording Counter, Type MU.1. Pp. 4. 'Cintel' Decimal Counter Chronometer. Pp. 4. 'Cintel' Mobile Photoelectric Counter Chronometer. Pp. 6. 'Cintel' Microsecond Chronometer. Pp. 2. 'Cintel' Standard Photoelectric Cells. Pp. 6. (London: Cinema-Television, Ltd., 1947.)

Gallenkamp Specialities. (Catalogue No. 508.) Pp. 60. (London: A. Gallenkamp and Co., Ltd., 1947.)